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SOME MINERAL RELATIONS FROM THE LABORATORY VIEWPOINT[1]

BY ARTHUR L. DAY

(Presented in abstract before the Society December 30, 1909)

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PURPOSE

Rocks have come to their present condition as the result of profound changes in physical structure and in the physical and chemical relations

[1] Manuscript received by the Secretary of the Society April 11, 1910.

of the component minerals. They therefore offer for investigation a number of problems of broad scope, requiring for their solution not only the application of the principles of geology, but also of physics, chemistry, and allied sciences. The problems are also complex in detail and do not admit of satisfactory analysis without correlating a great variety of evidence, both from the field and from the laboratory. The field evidence was, of course, the first to be sought; forces had to be recognized before they could be measured; but, having been identified, the next step is to seek to establish their relation to each other in the laboratory and to make use of the evidence from experiment for further and more accurate field studies.

Until recently, petrologists have confined their attention largely to the collection and examination of the field and microscopic evidence bearing on the rocks and their mode of occurrence, while the more precise quantitative methods of attack have been slower in development and are only now beginning to be considered seriously. This is especially true of experimental evidence, and one of the chief purposes of this paper is to invite the consideration of geologists, or, more particularly, of petrologists to certain phases of the problem of rock formation as they begin to appear from the viewpoint of the laboratory investigator, in order that men who are at work with a common scientific purpose, but with widely divergent training, may cooperate more effectively, both in gathering and interpreting the data on which to build the quantitative science of petrology.[2]

It will be well to recall that the tendency of scientific effort for a half century or more has been very definitely in the direction of more intense specialization. With perhaps an occasional exception, the men engaged in different branches of science have not been working along convergent lines, but along divergent lines, which are now so far apart that individual workers in one field do not find it easy to use the tools of another, nor at once to estimate the scope or point the application of the facts and relations established with these unfamiliar tools. It therefore happens quite naturally, in entering a new field of investigation such as that lying midway between geology on the one hand and physics and chemistry on the other, that the individual chemist, physicist, or geologist should feel that

[2] George F. Becker has repeatedly directed attention both to the need and to the opportunity for such cooperation. With Barus and King, he planned the first physical measurements made in the U. S. Geological Survey in the early 80's, and when these were interrupted in 1892 through failure of appropriations, it was his vigorous and continued interest which finally secured the reestablishment of the physical laboratory in 1901 and brought to it additional support from the Carnegie Institution of Washington in 1904. The introduction of the quantitative methods of physics and chemistry in the service of geology is therefore very largely due to Doctor Becker.

his personal experience does not adequately cover the whole ground. Neither is it desirable, in view of the enormous scope and detail of information necessary to a working knowledge of one of these sciences, that the investigator attempt to add to that an equally competent working knowledge of one or both of the others. It is a question whether such an effort would not be as likely to decrease as to increase his productive efficiency. It is rather to be regarded as a favorable opportunity for closer cooperation between workers representing different standpoints, when new and large problems are to be undertaken which lie in fields intermediate between existing branches of science or research. Large problems of industrial development have been effectively solved in this way, and there appears to be no fundamental reason for discriminating between the processes of gathering data for the advancement of knowledge and gathering data for commercial exploitation. The breadth of knowledge required and the necessity for exact information is certainly no less important in the first case than in the second. In approaching new problems which are accessible on two sides, it would appear, therefore, that the time has now arrived for effective combined effort of two or more men with appropriate equipment, rather than that any one should undertake them alone.

The Geophysical Laboratory is organized on this plan. It appeared to be sufficiently demonstrated that a comprehensive study of rock formation involves not only geology and mineralogy, but also physics and chemistry. The attempt was therefore made to perfect a working organization of men familiar with the methods of research in use in each of these fields. As a working system this has been most successful. It is therefore time to offer some of the results of such cooperative effort for more general consideration in order that we may also cooperate to make the most profitable application of the data thus obtained and become more familiar with the direction in which such investigation is leading.

Some Reasons for accurate Laboratory Work

Laboratory research in the service of geology has been attempted before by a number of different men, but with many serious interruptions and changes of viewpoint. Barus, Becker, and King proceeded chiefly from the physical side; Lagorio and the French scientists from the chemical; Vogt, more recently, from the standpoint of modern physical-chemistry. Barus laid great stress on the necessity for exact measurements and trustworthy data which could be applied quantitatively, but he has had few followers in this direction. It is, of course, an open

question, in entering a new field, whether progress is most effectively made by pushing forward rapidly with hastily gathered approximate data or by a slower procedure, with greater attention to the magnitude as well as the kind and direction of the forces operating. The first plan may, and indeed often has, led the investigator far astray; the second, with its slow advancement, taxes his patience as well as that of others who are waiting to make application of his results. Perhaps the choice should be left to the temperament of the investigator himself. Be that as it may, it has seemed wise, in organizing a permanent laboratory for the investigation of geophysical problems in a broad way, to adopt the slower procedure, on the general ground that the geologist who will use the data obtained by the laboratory will be unwilling to take the chance of being led astray by approximations, knowing that at the time when the approximations were made measured data could have been obtained with a little additional effort and the resulting confusion and revision of inferential conclusions avoided. Furthermore, errors in the investigation of minerals have the habit of becoming cumulative, and may very soon vitiate any kind of productive conclusion if allowed to remain large or if carelessly treated. The plan determined on was, therefore, to undertake a quantitative and thorough investigation of the properties of the chief component minerals and of the forces which are operative in rock formation, drawing on the resources of the exact sciences to any extent which might prove necessary. The present paper is devoted to the consideration of some of the phases of this problem.

Temperature Measurements are now Trustworthy and of adequate Range

The study of the crystallization of minerals from the magma has usually been begun by determining the melting or solidifying temperatures of the original component minerals on the supposition that the order of the melting points[3] and the character of those physical and chemical properties which can be studied in the vicinity of the melting point will provide one of the clues to the order of crystallization. Barus[4] recognized that the first step toward such a study was to provide an accurate high temperature scale in terms of which these melting temperatures could be ascertained and expressed, and to it he devoted several years of great activity. With the same purpose, five years of continuous study have been given to this problem in our own laboratory, and the results

[3] In the normal case of pure elements, or compounds, in equilibrium, melting and solidifying temperatures are identical. There are many apparent exceptions among the minerals, some of which will be treated on a later page.

[4] Bulletin No. 54, U. S. Geological Survey.

are just published.[5] As a result of these, temperatures in the vicinity of 1000 degrees centigrade can now be determined within 1 degree, and the probable error of temperature measurements does not exceed 2 degrees until 1600 degrees is passed. It is possible to continue such measurements beyond this point, but the uncertainty undoubtedly reaches 5 degrees at 1750 degrees, and perhaps 20 degrees or more beyond that. Such temperatures (above 1600 degrees) are rarely encountered in the operations more immediately concerned with rock formation, although the refractory oxides (CaO, MgO), when pure, melt far beyond 2000 degrees, and the determination of their properties, both alone and in certain combinations, has required occasional measurements in the region above 2000 degrees, with which we have encountered no serious difficulty. In respect of this absolutely indispensable factor in any quantitative study of mineral or rock formation, we are now assured of a scale of temperatures of sufficient range and accuracy to meet the most exacting requirement likely to arise.

Interpretation of Melting-point Data

On the other hand, the interpretation of melting temperatures when obtained is a matter about which still hangs a cloud of obscurity. On this subject the laboratory viewpoint has changed somewhat with increasing experience, and the changes affect the whole question of the experimental study of rock formation as at present understood.

In its first inception the original plan was to make a collection of typical minerals, as pure as possible, and to observe, on an appropriate thermometer, the temperature at which they appeared to melt. In so far as this procedure commended itself to investigators as simple, direct, and free from any probability of misinterpretation, it has proved disappointing and misleading, for the data gathered in this way have differed so widely in the hands of different observers, and even in the hands of the same observer at different times, as to lead us to consider most of the early observations uncertain and therefore untrustworthy.

The experience of the laboratory student is perhaps better calculated to provide an explanation of these differences than that of the mineralogist. To make the case as concrete as possible, I will therefore take the liberty to introduce some observations of these phenomena as they have been gathered in the Geophysical Laboratory from time to time.

[5] Arthur L. Day and J. K. Clement: American Journal of Science (4) 26, 1908, pp. 405-463; Arthur L. Day and Robert B. Sosman: American Journal of Science (4) 29, 1910, pp. 93-161.

Individuality of different Minerals in Melting

The laboratory student, proceeding from the physical standpoint, recognizes melting as a "change of state" involving, according to accepted molecular theory, a complete change in the molecular structure of the substance. Such a change of state will carry with it various visible evidences of its occurrence, as, for example, the disappearance of crystalline structure; a change of density; a change in the electrical conductivity; a more or less sudden appearance of fluidity causing it to take the shape of the containing vessel; a change in the specific heat—in a word, there appears a more or less conspicuous discontinuity in all its physical properties. In respect of these outward evidences, different substances will obviously vary.

In pure diopside, for example, the crystalline structure disappears with considerable promptness at the moment of melting, while silica and the more acid feldspars are extremely slow in transition from the one state to the other. The density change accompanying the change of state in anorthite is small, while for albite it is relatively much larger. If we assume that the relative density of the cooled products offers an approximate measure of its magnitude, the density change from anorthite to its glass is but 2 per cent, while for albite it is five times as great. In a substance like ordinary borax, which will no doubt prove to have several analogues among the minerals, the density change proceeds nearly as far in the opposite direction, while among organic compounds many are found to undergo no measurable change in either direction. Pure sillimanite flows so freely at the moment of melting that in an oxyhydrogen flame the liquid mineral is freely blown about by the flame, while pure silica and the feldspars again offer no outward evidence of flow during the melting process. Some of the phenomena attending the change of state are accessible to established methods of observation, while others can not readily be detected by any method suggested by existing laboratory experience. There is therefore a certain amount of individuality in substances which is altogether characteristic, but which may render a particular method which has proved fully competent with one substance to be very inefficient when applied to another. Considerations like these not only increase the scope of the laboratory problem, but detract somewhat from its assumed simplicity. If we would determine the melting temperature, or change of state, of a great body of substances, *it is not merely necessary to be able to measure temperatures accurately and conveniently, but also to obtain sufficient knowledge of the individuality of the substances under investigation to enable us to be quite sure that the method employed for detecting the change of state when it occurs is an appropriate one for each particular substance.* By way of illustration,

ORTHOCLASE FRAGMENT BENT AT 1200° UNDER LOAD (20 DIAMETERS)

A cleavage crack (dotted) has retained its orientation, although the fragment is melted through. The melted (dark) material is not squeezed out or otherwise displaced by the bending more than the unmelted original orthoclase.

one instance may be cited. More than one observer has sought to determine melting temperatures by watching for the moment when the substance appears fluid and runs in the crucible, and measuring the temperature at which this appearance of fluidity occurs. Moreover, this method has served successfully for the measurement of melting temperatures of low-melting salts and of metals, and will serve again in the case of single minerals[6] whenever it can be shown that this appearance of fluidity occurs coincidently with melting; but it so happens that in minerals this does not always hold true. A number of minerals, like quartz and the feldspars, melt to form liquids which are so viscous[7] that they show little or no tendency to take the form of the containing vessel, even after melting is complete, and therefore give no evidence, to the observer who merely watches for the mineral to run, of the beginning of melting. In the accompanying photograph (plate 3) a crystal of orthoclase was bent with a wire when partially melted. The amount of bending is shown by the cleavage crack. Dark areas are already molten; bright areas unmelted crystals. It is obvious that the melted (dark) material is not squeezed out or otherwise displaced by the bending more than the unmelted original orthoclase.

To the research student who has become experienced in such phenomena this plainly indicates that the particular property chosen, namely, the appearance of mechanical fluidity after sufficient heat has been applied, is not a suitable one with which to determine the melting points of substances which form viscous liquids, and that his ingenuity must devise a more appropriate means with which to approach these substances. In other words, experience teaches him that he must have more than one method at his disposal for the determination of melting points of minerals if he would obtain a competent record, and that he must make a study of each mineral from this viewpoint in order to assure himself that the method which he uses will locate the desired point without ambiguity in each individual case. In our experience, some minerals have been found to melt sharply to a thin liquid, and with these almost any method is competent to determine the melting temperature. Others can be more readily determined by noting the disappearance of crystal structure (indicated by abrupt changes in some optic property like birefringence) when a fragment of the mineral is observed in a furnace mounted on the table of a microscope. With still others the melting

[6] It will not serve for the study of a mixture of minerals, and is therefore of little or no value in approaching the subject of rock formation.

[7] Hyperviscous fluids differ essentially from solids. A fluid, unless extremely undercooled (glasses), yields continuously, though sometimes very slowly, even to the smallest pressures. A solid does not.

point can best be found by locating the temperature at which the latent heat of fusion is absorbed or released.

Uncertainty of Solidification—Undercooling

Let us carry the examination of the properties of different minerals somewhat further. We may undertake to determine the melting temperature of a particular mineral, and may obtain, as we suppose, a competent measurement, but on cooling again we note that the solidifying temperature falls at some distance below the melting temperature. From the petrological standpoint, the solidifying temperature will perhaps appear the more important of the two, and proceeding from this viewpoint alone, if these two temperatures should differ in the same substance, we might be tempted to reject the former and adopt the latter without special endeavor to obtain corroborative evidence of the significance of the difference. But here again we may mislead ourselves by an over-hasty conclusion. Laboratory experience has also shown us that minerals in which the solidifying point falls at a different temperature from the melting point, also display considerable differences between successive determinations of the solidifying point if the rate of cooling is changed or if the mineral, while melted, happened to be heated high above its melting temperature before cooling. In the explanation of this phenomenon there is valuable information for the student of mineral crystallization, if he will take the trouble to seek it; for it appears that minerals, much more than metals, possess the kind of molecular inertia by virtue of which changes of molecular arrangement, such as melting or solidifying, take place with some difficulty and slowly, instead of promptly and regularly, as metals do. Accordingly, as the liquid mineral cools down to its melting temperature, it may not crystallize at once, and, delaying, may cool for a considerable distance into the unstable region below the melting temperature before crystallization begins. Even after it has begun to crystallize the process may proceed so slowly, while the temperature continues to fall, that the visible evidence of a crystalline condition may not appear for some time longer. In such a mineral, obviously the crystallizing temperature is a property which can be determined only by the most careful observation, if at all. In all the pure minerals so far studied in the Geophysical Laboratory it happens that it can not be determined at all[7a]—that is, minerals, when cooling under atmospheric pressure, do not crystallize promptly at a particular temperature which is constant and charac-

[7a] Since the text of this article was written, accurate measurements of chemically pure pyrrhotite (by E. T. Allen) show the temperature of crystallization to occur promptly and to be identical with the melting point.

teristic of the substance, but at random, the temperatures being dependent on the rate of cooling and various extraneous conditions which need not be recounted here. It is even possible that such a substance, when cooled rapidly, may be so inert with respect to changes of molecular arrangement, or so viscous, that it may cool far below the melting temperature, becoming more viscous all the while, until finally it reaches the temperature of the room without having crystallized at all. Such substances are not uncommon; they are known, both to physics and to geology, as glasses, and they possess no molecular arrangement characteristic of the quasi-rigid condition which they appear to have reached. Strictly speaking, therefore, from the physico-chemical standpoint, such substances are undercooled liquids, and are still in an unstable condition, albeit opposing with overwhelming inertia (viscosity) any molecular rearrangement whatever. Proof of this lies in the fact that the latent heat of fusion which was absorbed during melting, and which is an absolute concomitant of the change of state, is not released on cooling to glass. In this condition days, or years, or even geologic time, may conceivably elapse before the characteristic crystal forms appear. In the extreme case where the crystallizing force encounters resistance of overwhelming magnitude, crystal formation is no doubt permanently stopped until new forces or conditions intervene to bring relief. Ordinary window glasses, or the obsidian cliffs of the Yellowstone Park, offer familiar illustrations of such permanently interrupted though still incomplete operations.

Some Difficulties of Interpretation where Glass is Present

The physicist, therefore, soon learns that there is much individuality among the characteristic properties of different minerals, and that determinations of their physical constants must proceed with unusual caution lest the result be misleading. In particular, having established the fact that many minerals do not solidify (crystallize) regularly at definite temperatures, he must, not always, perhaps, but usually, depend on melting-point determinations if he would learn the normal temperature of their change of state. He is also forced to take into account the fact that the phenomenon of melting does not have the same appearance with different minerals—that some change more conspicuously in one property, some in another—and that his observation of the moment of melting must be made with proper respect for the individual characteristics of the particular mineral under investigation. If a mineral has been found to melt to a viscous liquid, he may not set up a thin sliver, as was the common practice a few years ago, and observe with a telescope the first

appearance of rounded corners as a criterion that the melting point had been reached. It may have been passed long before. Furthermore, with respect to this particular illustration, if he has had wide experience he may reason that crystallization represents the operation of a force of characteristic magnitude operating between the molecules to maintain a given systematic arrangement (crystal form), while the rounding of the corners of a thin fragment represents the tendency of all molecular systems to reduce to the form which has the smallest surface—that is, a sphere. Bearing in mind that the effect of such a surface tension will be greater the smaller the sliver and the sharper its corners, without regard to its melting temperature, it will be conceivable that the rounding of the edges, which he has arbitrarily determined upon as his criterion, may represent the prevailing of one internal force over another, in which the observed temperature bears no relation to the melting point or change of state whatever.

The classic experiments of Barus to determine the change of density of a mineral upon solidification offer another illustration of the difficulty of determining the physical constants of minerals which cool to glass with little or no crystallization. All of his observations (save one) were made on cooling diabase, and he has left an unqualified record that the melting rock cooled to glass without crystallization in each case. There was therefore no discontinuity of molecular structure, no characteristic change of properties, no release of the latent heat of fusion—in short, no solidifying point whatever on the cooling curve, and therefore no temperature at which observations of density would have significance to the student of rock formation.[8]

Their Effect upon the early Measurements

All these observations of the peculiar properties of individual minerals in the vicinity of their melting temperatures which have been revealed by laboratory study subtract something, it is true, from the value of existing records, in obtaining which it is known that certain necessary precautions, of which an outline has been given above, were not recognized, and therefore not taken, but on the other hand they add immensely to our knowledge of the behavior of minerals during the process of crystallization, and offer positive reasons, where none existed before, for some of the anomalous observations which have been made on natural rocks, from

[8] The properties of mineral glasses were wholly unstudied at the time when Barus's experiments were made, but the sudden change in density, amounting to 3 per cent, which he obtained, is not explained by any known property which they have been found to possess.

which it appeared that the order of crystallization as seen under the microscope was not always the same, even though the chemical composition was approximately so. It is also possible for the first crystals which separated to redissolve at lower temperatures, so that the cold rock does not necessarily contain in itself a complete history of its crystallization. Such physical conditions as rate of cooling and the presence or absence of nuclei about which crystals might begin to form enter into this problem as determining factors. It is probably unwise, at this early stage in the development of the subject, to draw sweeping conclusions from these relations, except, perhaps, to emphasize the fact that the order of crystallization or differentiation from the magma will hardly be determined on the basis of chemical composition alone, though of course it must be fixed by the law of minimum potential.

Can trustworthy Melting Temperatures be obtained?

It remains for us to pursue the question on its constructive side, and to inquire whether trustworthy melting temperatures can be determined for the minerals in the face of the limitations which many of them have been seen to possess. Obviously not by the simple and heretofore generally acceptable process of using the same subjective criterion of the moment of melting for all minerals alike; namely, the change in the appearance of the mineral in a crucible during heating. Between the latitude which can be allowed to the judgment of the observer and the wide difference in the properties of the different minerals themselves, this method will fail, as it has done heretofore, to yield uniform and therefore trustworthy results on which to base serious geological conclusions. On the other hand, if we choose for each mineral an appropriate property which shows a conspicuous change corresponding to the change of state, or, still better, if we leave the personal judgment of the observer out of the question entirely, at least wherever it can be done, and measure by a sensitive method the change in the energy content of the system which is invariably coincident with its change of state, whatever the external evidence of the latter may be, we shall arrive, with nearly all the minerals, at perfectly definite constants, which may be redetermined at will, and which always possess physical significance.

Assuming that we have reached this standpoint, that the individual limitations in the properties of the minerals must be respected and that the judgment of the observer must be confined to conspicuous properties, or, still better, wholly eliminated, the analysis of melting-point deter-

mination becomes a straightforward question which can be treated along recognized lines.

Other Minerals lower the Melting Temperature

Most of us are familiar with the typical eutectic diagram as it appears in the text-books of physical chemistry and in the newer treatises upon rock formation (Vogt,[9] Iddings,[10] Harker[11]). This diagram tells us plainly that if to the component mineral (A) of simple and definite composition with a definite melting point (*a*) we add a small quantity of another mineral (B) (which forms no appreciable solid solution[12] with it), this melting temperature will be measurably lowered (to *b*), and a sufficiently large addition will bring it down to the lowest point of the curve which, following the physical chemists, we have come to call the eutectic (*c*). If our first mineral (A) is pure wollastonite, and we add 5 per cent of pure diopside (B), the melting point is lowered from 1510 degrees (pure wollastonite) to 1502 degrees. If more is added until the mixture contains 60 per cent of diopside and 40 per cent of wollastonite, the mixture will crystallize as a eutectic (*c*) at 1348 degrees (see figure 7, page 172). This is perhaps not the time nor the place for a long physico-chemical discussion of the conditions of vapor tension in a solution which result in the lowering of the melting point when one substance is mixed with another and melted. It can be found, with numerous illustrations, in any text-book of physical chemistry. The illustrations are drawn for the most part from the study of aqueous solutions at low temperatures, but

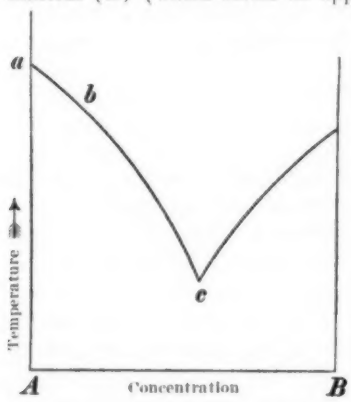

FIGURE 1. — *Diagram showing Temperatures of Melting and Solidification in a Eutectic Series*

[9] J. H. L. Vogt: Die Silikatschmelzösungen (2 vols., Christiania, 1903-'4); Physikalisch-chemische Gesetze der Krystallisationsfolge in Eruptivgesteinen, Tscherm. Min. u. Pet. Mitth., 24, 1905, pp. 437-542; 25, 1906, pp. 361-412.

[10] J. P. Iddings: Igneous rocks, 1909.

[11] Alfred Harker: The natural history of igneous rocks, 1909.

[12] Some minerals are able to take up a very limited quantity of another ingredient in solution so intimately that the second invariably crystallizes out with the first like an isomorphous pair (page 167), in which every crystal unit contains both minerals. Such crystals are called "mix-crystals" and such a solution *after crystallization* a "solid solution." When saturated in this way, its further behavior is independent and normal.

the quantitative studies of mineral solutions so far undertaken in the laboratory have brought to light no serious exception to these qualitative generalizations.[13] Quite the contrary. The enormous increase in the range of temperature required for mineral investigation compared with low-melting salts has afforded most satisfactory and greatly desired confirmation of the general validity of these relations.

Pure Types are Necessary

Without entering on a discussion of the principle itself, therefore, its applicability will be immediately obvious. If the addition of one mineral to another in any quantity, however small, lowers its melting temperature, and the amount of the lowering is considerable, this may be another thread which leads directly to the situation described earlier in this paper, namely, that mineral melting points gathered from all sources exhibit differences sufficiently large to make them wholly untrustworthy. Any change in the composition, even if small, places the melting point on a sliding scale of temperature so steep (the curve *ac*, figure 1; also AB, figure 7) that considerable differences in the measured melting points must inevitably result. Such observations reflect only too faithfully the fact otherwise established in many different ways, that typical minerals are not found without small and variable admixtures of other minerals which affect their melting temperatures differently. This is in no sense a limitation on the study of the characteristics of minerals during formation, or the competence of such studies, undertaken in the vicinity of the melting temperature, to reveal their true relation, but it imposes on us the absolute necessity of first establishing some quantitative relation between the *kind and amount of impurity and the kind and amount of effect produced by it*. To a certain extent also we may determine relations between some natural types (like eutectics) which have important factors in common, but we can not recognize and quantitatively establish the interdependence of those qualities which determine types if they can not first be separately observed. To the laboratory student this is merely a particular case of the general working rule that the number of unknown factors in his system must not be greater than the number of measurements available for their determination. He is undertaking to determine not only the character of the operative forces in rock formation, but their

[13] It is perhaps well to warn the student against the mistake of trying to apply to rock formation the *quantitative* laws of solution which have been deduced for very dilute *aqueous* solutions, and which can not yet be applied to concentrated aqueous solutions, much less to viscous concentrated silicate solutions, at least not until the various participating factors have been definitely evaluated, which has never been done.

magnitude, and to do this successfully requires that each variable shall be capable of independent determination. Without these precautions it will be impossible even to distinguish those forces and components which are essential from those which are merely incidental to his problem.

Preparation of pure Minerals

This conclusion might perhaps have been reached *a priori*, but in common with many conclusions of like fundamental significance it has waited to be brought out by a long and painful experience of experimental disappointment. But our attitude toward the new science must be constructive, and not destructive. If the accumulated experience of years has gone to demonstrate that quantitative experimental petrology depends on obtaining the minerals separately—that is, chemically pure—and determining their individual and characteristic properties, and afterward on our ability to combine them in known relations, the question which presses hardest for answer is obviously whether or not chemically pure minerals can be obtained for laboratory study by practicable processes. To this most vital question all the experience thus far gathered by the Geophysical Laboratory goes to establish the affirmative answer. Not much is to be expected from a more diligent search for purer natural types than those already gathered; neither is there much encouragement for the successful purification of natural minerals by laboratory process with the single exception of quartz, but mineral synthesis from chemically pure ingredients has been almost uniformly successful.

Here again the difficulties encountered by the laboratory in the synthetic preparation of minerals contain information for the student of natural formations. Not all molten minerals, as we have seen, can be made to crystallize within the limited time available in the laboratory. Such important minerals as quartz and the alkaline feldspars will not crystallize in a furnace at any temperature unless volatile ingredients are present to give the required molecular mobility, and thus to facilitate molecular rearrangement. With the help of such volatile constituents, pure quartz crystals can be obtained of a size and perfection sufficient for determinations of high accuracy. It therefore suggests itself quite naturally that we are approaching rather than departing from natural processes in crystallizing quartz out of a solution containing volatile ingredients which participate in the formation process, but do not appear in the finished product. There is abundant field evidence that vein quartz has at some time possessed mobility sufficient to enable it to penetrate into the minutest cracks, and yet but insignificant traces now remain of any other

components. This same vein quartz, when melted in the laboratory, is among the most viscous of minerals, and takes the form of the containing vessel only when placed under stress. It is, therefore, more than probable that it once contained ingredients which contributed enormously to its fluidity during solidification, but of which only a trace is left—that is, volatile ingredients, like alkaline water solutions, or fluorine; the same materials, in short, which produce the same result in the laboratory.

Another difficulty arises through the tendency of rapidly crystallized laboratory products to come out in a cryptocrystalline condition. If the short time interval available for laboratory experiments is insufficient to provide opportunity for the formation of large units, may it not also be true that the size of grain in natural formations gives some indication of the rate at which the formation went on? An inference of this kind must, of course, be applied with care if volatile ingredients are present, for small differences of composition often produce relatively very large differences in the fluidity of the magma, and therefore in the size of the crystal grains. In fact, the introduction of volatile ingredients is the expedient used in the laboratory production of minerals which tend to crystallize only in the most minute units.

As a record of laboratory experience, it is unnecessary to do more than call attention to the character of these difficulties which the preparation of the pure minerals in the laboratory encounters, and to say that none of them has yet proved insuperable. Nearly all our publications contain illustrations of the practicability of obtaining pure mineral types of sufficient perfection for competent study. Given these pure types, and there is no difficulty of principle in establishing properties and relations and erecting on them a quantitative system of solutions corresponding to the typical natural rocks. In practice, the development of such a system is slow, for the number of participating components in natural rocks is very large, and the complications accordingly intricate. The solution of the difficult cases must await the gathering of a wide range of laboratory experience and a considerable elaboration of existing theory. On the other hand, the attempt to build a physico-chemical system on observations of natural minerals alone will surely resemble an attempt at triangulation without fixed triangulation stations.

All governing Conditions must be known

If the first essential to the successful study of the properties and relations of the minerals in the laboratory is the ability to obtain pure types, the second is certainly the necessity of defining the conditions under which

these types shall be examined in the vicinity of their formation temperature. Here the experience of the laboratory points out two situations which lead to confusion. The first is the fact, not hitherto mentioned, that the addition of impurity, even of minute proportions, to a pure mineral not only has the effect of displacing its melting point, but also of stretching it out along the melting curve in such a way as to conceal its exact location upon the temperature record of the thermometer. The second is the frequent difficulty in establishing equilibrium during the operation under investigation.

Melting "Points" and melting Intervals

Among the somewhat voluminous records of mineral melting points which have come out of European laboratories in recent years, one feature has reappeared persistently, namely, the inability of the observer to fix upon a single temperature for the melting point of a mineral under observation; instead, he almost invariably finds and attempts to fix the boundaries of a melting interval, often of 30 or 40 degrees (actually 50 degrees in the adjacent feldspar curves (figure 2) measured in this laboratory), throughout which the phenomenon of change of state appears more or less continuous. Physical and physico-chemical experience offers reasons for this behavior which may in part explain, but which do not always help to remedy the difficulty. If the material under investigation is a solid solution or isomorphous mixture (as in the feldspars), we know that the phenomenon of melting includes a continuous change of composition which is actually distributed over a considerable range of temperature, so that the appearance of slow melting is not here misleading, but is the correct record of its actual progress. The true relations here require to be ascertained by establishing, either through chemical analysis or the microscopic examination of a thin section of the material after solidification, the order in which the crystals of different composition have formed and the limits of variation in their composition.[14]

If, however, the substance under investigation is a chemically pure compound the composition of which does not change during the change of state, the reasons for the change being distributed over a temperature interval are of a more accidental character, and should in most cases be capable of elimination by careful study. Suppose, for example, we ex-

[14] The general cases of this kind (see also page 166), together with various exceptions encountered in the study of the feldspars, were discussed, both from the practical and theoretical standpoint, in the record of the first investigation made in the Geophysical Laboratory, from which figure 2 is taken. (The Isomorphism and Thermal Properties of the Feldspars, Publication No. 31, Carnegie Institution of Washington.) The discussion is too long and too severely physico-chemical for reproduction here.

amine the distribution of heat in a crucible containing a pure mineral whose melting point requires to be determined. In a cylindrical furnace in which heat is supplied to the charge from the sides (the usual case, figure 3) its outermost layer will be the first to receive heat, and through this layer heat will be transmitted to the inner layers somewhat slowly, for most minerals conduct heat but poorly when compared with familiar metals. The same analysis shows that heat leaves the crucible for the

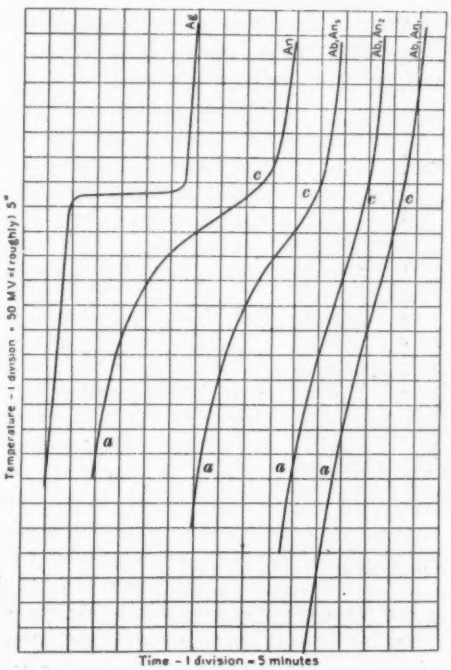

FIGURE 2.—*Melting Curves of the Feldspars*

Taken from Day and Allen: "The Isomorphism and Thermal Properties of the Feldspars." Publications of the Carnegie Institution of Washington, No. 31. The melting intervals are (roughly) included between the letters *a c* of each curve. A typical metal melting curve (silver) is included for comparison.

most part through the top and bottom layers in the same order. At the beginning of melting around the periphery, therefore, the center of the charge has a lower temperature, and has not begun to melt. The exposed upper surface is colder still. If the material has a very high melting point these gradients may be steep. Suppose that we have decided not to depend

on the appearance of the exposed surface in the crucible, and are working with a closed furnace and a sensitive thermometer imbedded in the charge which will record the temperature of the portion of the charge in contact with it; melting will then begin in the outermost layers (figure 3), and will proceed gradually toward the thermometer at the center. Here the temperature record will show heat absorption from the beginning to the end of the melting process, but only the melting of the innermost layer which is in immediate contact with the thermometer gives the true melting temperature of the substance. Such an observation gives the appearance of a melting interval where none really exists. It can be avoided by simply omitting the outer layers—that is, by using a much smaller quantity of material in the form of a narrow cylindrical charge. If, instead of using a crucible of conventional form, we reduce the amount of material to 1 or 2 grams in a crucible about the size and shape of a lead pencil, and of perhaps 1 inch in length (figure 4), the same melting point, which appears much displaced and distorted upon its curve in the large crucible, will appear and reappear with the most satisfactory sharpness with the small one, with the added advantage that the experimental operations all become much simpler. By this simple expedient measurements of the melting temperature of pure diopside have been repeatedly made which agree within 1 degree at 1,391 degrees.

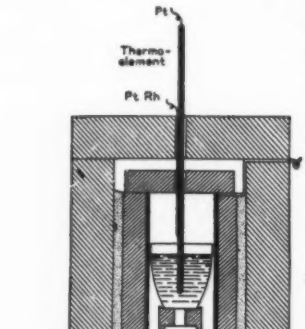

FIGURE 3.—*An Electric Furnace of usual Type with Crucible and Thermoelement in Position*

Substantially the same effect is produced by the presence of a small amount of impurity. Traces of heat absorption, indicating the beginning of the change of state, appear on the heating curve far below the melting temperature of the pure substance, owing to the solvent action of the impurity and consequent change in the specific heat of the mixture. This disturbing effect may be serious, or it may be insignificant, depending on the thermal properties of the substances involved, their amount, and mutual solubility. If the amount of impurity is very small (a few tenths of a per cent), the absorption of the heat of fusion will proceed rapidly to a reasonably constant maximum, the interpretation of which will not be difficult after a few trials with widely different rates of heating. Very rapid heating minimizes the apparent disturbance, but may (in viscous

substances) superheat the whole charge. Small and narrow crucibles (figure 4) have the effect of diminishing this difficulty also. This whole question of apparent anomalies in the determination of melting points due to small quantities of impurity and irregular heat distribution has been treated in considerable detail, both theoretically and practically, by W. P. White in two recently published papers.[15]

The third difficulty with the sharpness of melting points is inherent in the participating materials themselves, and the temperature-measuring device is merely offering a faithful interpretation of what actually occurs.

FIGURE 4.—*Small Crucible for the accurate Determination of the Melting Point of Minerals (nearly full size).*

Earlier in the paper it was noted that a liquid mineral on cooling often does not crystallize promptly, but through the influence of other properties, chief among which is the viscosity, the crystallizing temperature is lowered by an uncertain amount. This was explained as a kind of molecular inertia, inherent in certain substances, through the operation of which any molecular rearrangement, such as crystallization from a molten mass, is seriously hindered. Now it sometimes happens that what is true of crystal formation in a cooling mass may also be true of crystal destruction in a melting mass—the crystalline structure can only be broken down with considerable difficulty and delay. In such a case the expenditure of the energy required to break down the molecular system (latent heat) will in fact be distributed over a temperature interval, and will be so recorded by the thermometer. Just as in the case of the delayed solidification, where the solidifying temperature appeared low and variable with the rate of cooling, so here the delayed melting may result in too high a temperature, which is equally dependent upon the rate of heating. In extreme cases, of which quartz and albite again serve as familiar examples, this effect is so pronounced that no melting "point," in the proper sense of the word, can be obtained—that is, the change of state itself actually extends over a greater or less temperature interval, and the accurate interpretation of such a melting curve is a matter of some uncertainty. A series of actual curves showing the long temperature interval (100 to 200 degrees) over which the heat absorption of melting orthoclase is distributed, with different rates of heating, is shown in figure 5. The dotted line indicates the interval during which heat is absorbed. In

[15] W. P. White: Melting-point determination. American Journal of Science (4), 28, 1909, pp. 453-473. Melting-point methods at high temperatures. American Journal of Science (4), 28, 1909, pp. 474-489.

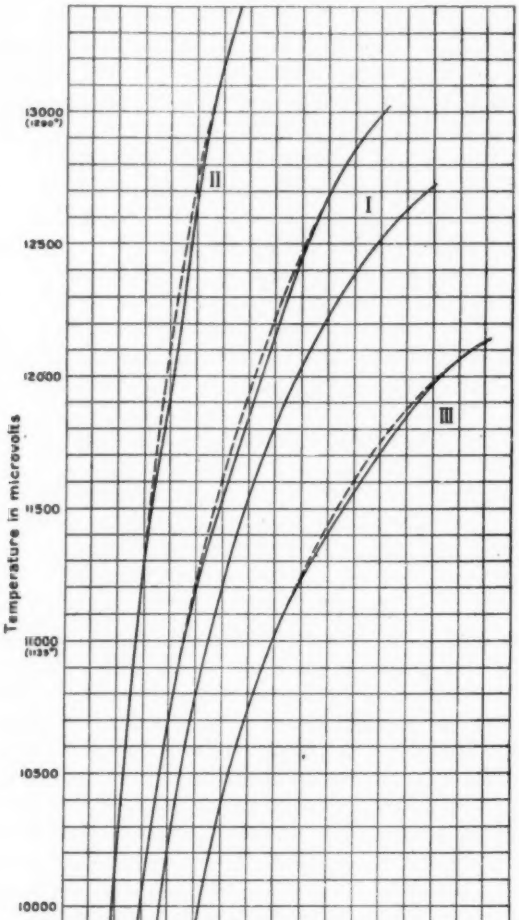

FIGURE 5.—*Melting Curves of Orthoclase*

The area included within the dotted line represents the approximate distribution of the latent heat of fusion for different rates of heating. (From Day and Allen, loc. cit.)

such cases equilibrium[16] is not established during melting or solidification, and no "point" can be found which possesses greater physical significance than any neighboring point. To assign especial significance to any single temperature is, therefore, wholly arbitrary. If the interval is short, its limits can be determined approximately; if long, there is little of physical fact with which to determine them accurately.

Significance of "Equilibrium"

The uniform experience of the laboratory has been that where equilibrium can be established during melting or solidification—that is, where the molecular system is not too inert to react with reasonable promptness to changes of temperature and pressure—there is a melting temperature which is constant for the particular substance, which is absolutely characteristic of that substance, and which can be determined. Such a melting temperature is independent of the rate of heating and of other superimposed conditions. On the other hand, there are minerals (of which quartz and albite were cited as examples) of such molecular inertness that they do not reach equilibrium in the time available for a laboratory measurement, and can not be made to do so unless the volatile ingredients which must have participated in their natural formation can be restored.

Effect of Pressure

A number of experiments have already been undertaken in the laboratory which serve to show that such a restoration is entirely practicable, and which incidentally show the operating forces of nature's laboratory in a more normal relation to each other. The field is not yet sufficiently developed for more than a suggestion of the conditions which prevailed in nature during the cooling of mineral masses containing volatile components in solution, but the suggestion is worth noting. To the petrologist seeking to obtain exact information upon the behavior of his minerals during heating and cooling, it has been somewhat puzzling to learn from the laboratory that pure silica is not known to melt below 1600 degrees, while his field observations clearly show that most observed natural quartz must have crystallized considerably below 800 degrees. It has been equally

[16] By equilibrium is meant the situation in which all the operative forces so balance each other that the system would remain indefinitely exactly as it stands, provided no change is made in the pressure or temperature or composition. As soon as equilibrium is established, physical measurements can be made with certainty and intelligently interpreted. Measurements made in transition periods under indeterminate conditions are very difficult to interpret at all.

puzzling to the laboratory student to hear the petrologist ascribe the low formation temperature and several physical characteristics of rocks to the effect of pressure, when he knows that pressure has relatively little effect upon a single pure mineral at any stage in its formation process. Thirdly, as has been noted before, the laboratory observer reports that liquid silica and many acid silicates are enormously viscous, while the field observer finds abundant evidence of extreme fluidity during their crystallization in nature. All three of these difficulties of deduction become clearer immediately we take into account the effect of volatile ingredients. The melting temperature, as we have seen, is much lowered by such substances, the viscosity is enormously reduced by them, and finally, the pressure is necessary to hold these volatile ingredients in solution. The very fact that the pure silica of the laboratory, or a pure natural silica, behaves in a way so different from the obvious behavior of the same substance during its initial crystallization in nature is of itself proof that its behavior has been influenced by the action of other substances, and if these are no longer present, or have left mere traces, we must assume that they were volatile and endeavor to restore them. This is the next step in the laboratory problem, and one in which supporting field evidence will be of the greatest assistance.

Single Minerals—Summary

In the preceding pages the effort has been made to describe some of the properties of single minerals near their melting temperatures as they have been developed by laboratory study. It has been more or less inevitable that emphasis should be laid on what may be called the disturbing elements like the effects of viscosity in preventing the establishment of equilibrium during the change of state, and so delaying, or even preventing, crystallization from the liquid, in concealing the outward evidence of melting, or in producing cryptocrystalline masses. The effect of small amounts of impurity in lowering the melting temperature and preventing the accurate interpretation of melting-point determinations and the effect of the relatively poor heat conductivity in causing considerable local irregularities of heat distribution in the mineral under observation have been frequent causes of misunderstandings and misinterpretation in laboratory study. Little has been said of the successful work of Barus and others in establishing the temperature scale which is absolutely indispensable to any quantitative research in this field.

It has been shown that these difficulties which have been encountered by the laboratory have merely served to develop important phases in

natural rock formation, and are, therefore, an essential part of the problem. The disappearance of the volatile ingredients from extruded magmas has left them viscous and caused obsidian formations. Their presence lowers the formation temperature, gives mobility to the fluid magma and larger individual crystals. It is through the effects produced by the volatile components also that one of the chief effects of pressure as a controlling force in rock formation has been brought to light; the pressure serves to retain the fugitive ingredients in the rocks, and these are probably chiefly responsible for the low formation temperatures. No doubt the field experience of the petrologist will suggest to him many other relations which result from the same causes.

Mixtures of Minerals

It now remains for us to consider mixtures of minerals from the laboratory viewpoint. Given a homogeneous mixture of two minerals in the molten condition, contained in a suitable crucible, and suppose them to be in complete equilibrium for the prevailing temperature. We will also suppose that neither of the components will boil within the temperature range of investigation, and therefore that the effect of pressure is insignificant and negligible. If this mass is gradually cooled, a temperature will soon be reached when the solution will contain more of one of the ingredients than it can hold in equilibrium. Now, whether it will crystallize or not will depend primarily upon two opposing forces, the power of the molecules to arrange themselves in the order characteristic of the particular mineral now in excess, and the power of the viscosity or some other opposing property to prevent this or any other rearrangement, exactly as in the case of a single mineral. If the viscosity prevails, the tendency of the excess component to crystallize is not strong enough to overcome the opposing force, crystallization is delayed, or maybe indefinitely deferred, and the excess component cools without displaying any evidence of its relation to the rest of the solution, for viscosity increases as the cooling goes on, so that if the excess mineral does not crystallize at an early stage it may find no better opportunity farther down. Thus our whole solution may cool gradually from its initial temperature to the temperature of the laboratory, without anything whatever happening within it to distinguish one component from another or the whole mass from any other mass of random composition. The solution merely persists as a liquid (glass) beyond the temperature where the ingredients should separate, and reveals none of its characteristics.

The eutectic Relation

The second possibility when the cooling solution reaches the temperature where one component is in excess has not yet been definitely proved to exist among the minerals, but it has been studied by Miers and Miss Isaac[17] with a view to such application, and is included in all the major treatises[18] on physical chemistry, with a variety of illustrations from aqueous solutions at low temperatures. According to this view, although one of the minerals is now in excess and free to crystallize out, the equilibrium is of a metastable or neutral kind, so that the actual crystallization of the mineral can only be precipitated by introducing solid crystal fragments of the same mineral (or another isomorphous with it) from without. These fragments, which may be minute in magnitude and few in number, provide the crystal nuclei about which the molecules of the excess component immediately group themselves. No other variety of crystal or any form of mechanical disturbance will cause the separation. The range of temperature over which this attitude of indifference to all but its own kind obtains is relatively small and differs for different substances. After passing through this metastable region the excess mineral enters a region of labile equilibrium where crystallization of the unstable component may be expected to be rapid and spontaneous, and to continue in step with the cooling until the eutectic temperature is reached. Here the other component is due to crystallize side by side with it, and will do so unless delayed for a longer or shorter time by similar limiting conditions.

In practice, at high temperature it is difficult to distinguish this case from the preceding one (see Harker, loc. cit., page 209), where crystallization is merely delayed by viscosity or some form of molecular inertia. Of course, the same situation may equally well arise among pure minerals crystallizing alone.

The third possibility is the normal one in which one component begins to crystallize out the moment the cooling has reached a point where it is present in excess, and equilibrium prevails throughout the whole process of crystallization. The eutectic also appears promptly, and the entire operation follows the simple diagram on page 152. Curiously enough, no

[17] Miers and Isaac: Journal of the Chemical Society (London), Transactions, 89, 1906, pp. 413.
Miers: Science Progress, 2, 1907, pp. 121. (Also several other papers.)

[18] The best statement of this case is by Ostwald: Studien über die Bildung und Umwandlung fester Körper: 1, Übersättigung u. Überkaltung, Zeitschr. f. phys. Chem., 22, 1897, pp. 289.

certain case of this most formal type of crystallization has yet been established with certainty among the minerals. As in the case of a single mineral cooling down after melting, there is always *some* undercooling[18a] from one or other of the causes above outlined, and crystallization is more or less delayed. It is, however, altogether probable that the basic minerals which are more thinly fluid will furnish numerous illustrations of it.

It is not necessary that the reader infer from the absence of explicit cases of normal behavior in the crystallization of mixtures of pure minerals so far studied that we are on the wrong track, and that the existing theory of solutions and the phase rule can not be properly applied to mineral solution, for the converse case of a mineral mixture melting normally has been frequently observed and is unmistakable. The eutectic usually melts promptly for all compositions in which it is present in sufficient quantity for observation, while the excess component enters the solution gradually, and disappears at the temperature appropriate to its concentration. It is a matter of great good fortune to the investigator endeavoring to establish quantitative relations between the minerals, that although individual physical properties frequently intervene to prevent the normal phenomena from appearing promptly on the *cooling* curve, the *heating* curve is rarely disturbed by them.

In this connection, attention may again appropriately be called to a phenomenon first mentioned in a publication from this laboratory on the crystallization of feldspars,[19] in which it was pointed out that in viscous solutions which are not stirred, rapid heating, and even more conspicuously heating but a short distance above the melting temperature, frequently affects the behavior of the solution during subsequent cooling. When crystals are melted, their molecules frequently remain in the same relative positions in the liquid, "as motionless as a school of minnows in a brook,"[20] for some time, and for a considerable temperature interval (a hundred degrees or more) above the point where melting is complete. In time and at high temperatures the schools are scattered, but recrystallization on cooling is much assisted if the cooling takes place before these schools have become dispersed.

Of course, the number of such individual mineral characteristics or properties which enter into the quantitative determination of its relations to other minerals in the laboratory is greater for a two-component system

[18a] See footnote [7a], page 148.

[19] Arthur L. Day and E. T. Allen: Isomorphism and thermal properties of feldspars. Publication No. 31, Carnegie Institution of Washington, p. 54.

[20] As Doctor Becker first suggested to me.

than for a single mineral, and much greater when three components are brought together; but they have not been found to differ in character from those offered by way of illustration in the preceding discussion of the behavior of a single mineral when heated. Their increased number invites caution and greatly enhances the value of laboratory experience in prescribing the conditions which shall surround the measurements, but they are in no sense prohibitive, nor do they threaten to become so. Beyond this their elaboration is more properly left to discussions of particular cases in which they occur, of which the literature of the Geophysical Laboratory already contains several.[21]

The Isomorphous Relation

The crystallization of a two-component mixture in which the component minerals are in isomorphous relation to each other is of somewhat different character. Here, again, the general case may be found in any desired detail, and with appropriate illustrations in general treatises on physical chemistry and in the more recent handbooks of petrology (Iddings, Harker—loc. cit.); also in the publications of Vogt (loc. cit.), Doelter,[22] and the Geophysical Laboratory.[23] Some particular phases of the subject are brought out by laboratory study which do not immediately appear in the investigation of natural minerals with the microscope, and there is little in the low temperature researches of the physical chemist to suggest looking for them. The cooling of an isomorphous mixture of definite proportions (for example, a lime-soda feldspar), even in the simplest case of unlimited mutual solubility and no intermediate compounds, offers at least two cases which the petrologist will find it necessary to differentiate—the case in which under-cooling occurs, and one in which it does not. If under-cooling does not enter (the simplest theoretical case), a solution of A and B (figure 6) of composition *a*, cooling from a molten condition to the temperature represented by *b*, begins to separate at once, but not in crystals of one of the original components, as in the case of a

[21] Day and Allen: Isomorphism and thermal properties of the feldspars, loc. cit.: Arthur L. Day and E. S. Shepherd: The lime-silica series of minerals, American Journal of Science (4) 22, 1906, pp. 265-302; E. T. Allen, Fred. Eugene Wright, and J. K. Clement: Minerals of the composition $MgSiO_3$; A case of tetramorphism, American Journal of Science (4) 22, 1906, pp. 385-438; E. T. Allen and J. K. Clement: The role of water in tremolite and certain other minerals, American Journal of Science (4) 26, 1908, pp. 101-118; E. T. Allen and W. P. White: Diopside and its relations to calcium and magnesium metasilicates, American Journal of Science (4) 27, 1909, pp. 1-47; E. S. Shepherd and G. A. Rankin: The binary systems of alumina with silica, lime, and magnesia, American Journal of Science (4) 28, 1909, pp. 293-333.

[22] C. Doelter: Physikalisch-chemische Mineralogie, Leipzig, 1905.

[23] Day and Allen, loc. cit.

eutectic pair. The first crystals to appear are of mixed composition, somewhat richer (*cd* in the figure) in the higher melting component (A) than the original composition, and the remaining solution is in consequence left somewhat poorer in this component. These first crystals will be followed by others containing less and less of A than *cd*, until the mixture finally ends its crystallization with mixed crystals of a composition well over toward B. In the figure, the lower curve represents the composition of the crystals which form, and the corresponding points (found by drawing horizontal lines through the points desired) of the upper curve, that of the solution left behind. It is a process in which the compositions change continuously throughout, and obviously the heat distribution also. There is, therefore, no sharp melting or solidifying point. Whether the resulting crystals will vary continuously in composition from *c* to *e*, or will appear in broad or narrow bands with insignificant transition stages, or will even come out homogeneous and of composition *f*, will depend in part upon the characteristic properties of the ingredients and the nature and extent of the disturbances to which the cooling system is subjected. All these cases are fairly common in igneous rocks. It should be borne in mind, however, that if the solution is of a character to follow promptly all changes of pressure and temperature as they occur, no bands will appear. The appearance of bands is direct evidence of slow transitions and disturbed conditions of cooling; their common occurrence in the rocks is indicative of frequent and considerable interruptions during the cooling process.

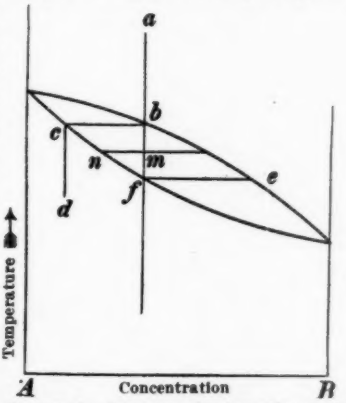

FIGURE 6.—*Diagram showing Change in Composition during Melting in an Isomorphous Series*

If under-cooling occurs, no crystals separate when the solution reaches the temperature *b*, but cooling continues along the line *bf* for a greater or smaller interval, as the case may be. If it were to begin crystallizing at *m*, the process would be exactly the same as before, except that the range of possible compositions would be smaller—that is, there would be no crystals as high in (A) as in the previous case (the first crystals would have the composition *n*). If the solution reaches *f* or cools below it, without the formation of any crystals, it will crystallize (if at all) in

homogeneous mix-crystals of that composition. This was the case with all the lime-soda feldspars crystallizing in the laboratory. Crystallization did not begin until the molten mass had passed entirely through the region in which the changes in concentration occur, and all the crystals and the glass residue, if any was left, were of one concentration, corresponding to that originally taken. We made no effort in this investigation to produce the banded structure in imitation of nature, on account of the time required to do it and the fact that it would add but little to our knowledge of the subject. All that is necessary is a furnace in which the temperature can be held constant within the temperature region where the changes of composition occur and sufficient patience to wait for the results (with the intermediate feldspars, probably some weeks).

Simple Mixtures—Summary

These simple cases, very briefly stated, serve to illustrate the behavior of cooling mixtures of two components. In the first case (the eutectic series) one of the components first appears separately, followed by a mixture of both in fixed proportions, called the eutectic; in the second (the isomorphous series), the two components always appear together, the first crystals to form being usually considerably richer in the higher melting component, followed in gradually changing proportions by the lower melting mixtures. Both these cases may occur in the same series of mixtures. For example, if the metasilicates of lime and magnesia be mixed together in a number of proportions and studied after the manner outlined above, it will be found that a compound (diopside) forms in the middle of the series. On one side, this compound forms a simple eutectic series with pseudo-wollastonite; on the other, a partial series of mix-crystals with the magnesian metasilicate. It is not uncommon to find two or three compounds forming between two components, with an appropriate number of eutectics between. Lime and silica, for example, yield two compounds, a metasilicate and an orthosilicate, and eutectics occur between pure silica and the metasilicate, between the metasilicate and the orthosilicate, and finally between the orthosilicate and pure lime. In this last series (orthosilicate-pure lime), not only both components, but the eutectic itself, melt at temperatures above 2000 degrees.

Molecular Composition at the Moment of Crystallization

There is opportunity, in a series like this, to study a situation to which Harker has called attention especially, namely, that the molecules of the liquid magma are not the simple oxides (SiO_2 and CaO), but are silicates of a composition appropriate to the percentages present in the solution

($CaSiO_3$ and Ca_2SiO_4), except near the ends of the series, where the pure oxides remain over in excess. If combination occurred only at the moment of crystallization, the accompanying energy change would be of a different order of magnitude, according as the component appearing in excess is one of the original oxides or one of the silicate combinations.

Characteristics of the cooled Products—some Definitions

The next important step, after differentiating the various situations through which a cooling magma passes, is to learn to recognize the distinctive characteristics of the products which result—the glasses, isomorphous mixtures, eutectics, and new compounds.

Up to the time of Lagorio but little attention was paid to the glasses in the study of rock synthesis. Lagorio endeavored to make use of them as hypothetical standard solvents in which various oxides could be dissolved to form rocks of different character, making it the counterpart in the mineral world of water in aqueous solutions. Each rock was to be considered as a mineral or group of minerals in solution in one of two common solvents. This viewpoint laid much emphasis upon the importance of rock glasses, but it has not yet contributed materially to the elucidation of rock formation. Doelter has endeavored to ascribe individuality to glasses, and has experimented with a great many glasses of different compositions without arriving at any conclusion of general interest. Barus, in his determinations of physical constants, esteemed it a matter of secondary importance whether a mineral cooled to a glass or crystallized. Modern physical chemistry ascribes little individuality to glasses as such, but considers them merely as under-cooled liquids which can be crystallized only with the greatest difficulty, or not at all, and which in consequence possess only tentative and general properties. They accordingly possess about the same significance as any other incomplete reaction which is caught in transition between states. It is much more important to be able to define these end states and their relations than to be able to fix the progress of the interrupted operation in individual cases. We find obsidians in which some crystallization has occurred, and rocks in which but a small portion of the total content is glass, all of which merely serves to show that the progress of individualization of a magma may be interrupted anywhere whenever the cumulative action of the opposing forces has reached a sufficient magnitude. It may also be resumed on the advent of new, favorable conditions, such as would be produced if a supersaturated magma were to come in contact with crystals of the excess component.

An isomorphous mixture, as its name indicates, is an intimate (crystalline, not vitreous) mixture of two or more component minerals in which

the physical properties change continuously with the composition. Isomorphous mixtures are most commonly found between minerals of closely related atomic or molecular grouping—that is, of similar chemical composition or crystal habit. The concentration may vary slightly or considerably in the same hand specimen, depending on temperature or other changes during its crystallization. Its distinctive feature, which is familiar both to the field and to the laboratory student, is the *continuity* of the changes of its physical properties with the changes in percentage composition of its ingredients. It may be added, for the sake of clearing up a possible misunderstanding, that the term "solid solution," as defined by the physical chemists, applies to *crystalline* solutions of this kind, and not to the glasses, as is sometimes stated. The glasses from this viewpoint are still ultra-viscous liquids, and retain the "latent heat of fusion" which would have been given off had a change of state occurred.

The eutectic is a mixture in which the component minerals bear a definite relation to each other, but neither loses its identity. It must not be regarded as in any way analogous to a compound. It has been commonly supposed that the simultaneous crystallization of two minerals in eutectic relation always resulted in a peculiar close-grained interweaving of the crystals to which the name "eutectic structure" has been applied. Such a "eutectic structure" is usual in metallic alloys and is readily identified, but pure minerals cooling under laboratory conditions do not show it. Whether the peculiar interweaving observed in graphic granite is a true eutectic structure of quartz and feldspar, or merely another of the effects of an earlier intermixture of certain volatile ingredients no longer found there, can not receive a positive answer. Such laboratory experience as we have appears to indicate that this structure is the result of a much more complicated activity than the mere cooling of quartz and feldspar in eutectic (?) proportions. The appearance of a eutectic in a solution of two minerals can be recognized in a variety of ways, but it is extremely doubtful if any *structure* has yet been found to be *characteristic* of the eutectic relation between minerals.

The appearance of a compound in a series of mineral mixtures breaks up the series abruptly into two, either of which may possess any of the characters above outlined. These may be treated quite independently of each other for purposes of systematic study of any kind. The compound possesses independent physical and chemical properties which can be determined for the purpose of distinguishing it from all other compounds, *provided* it can be separated out and independently studied. The mere appearance of particular optical properties in the grains of a rock section, for example, while it may suffice for the immediate identification of a known mineral, is not sufficient for the establishment of a new one,

A compound must be separated for purposes of definition. New compounds are not uncommon products of the laboratory study of minerals at their formation temperatures. Many of these are high-temperature minerals (like pseudo-wollastonite) which have no counterpart in the natural rocks. Had the natural rocks crystallized without water or low-melting alkalies, we should no doubt have had them, for some of them crystallize with great promptness from appropriate compositions, and are perfectly stable over considerable ranges of (usually high) temperature.

Eutectics in complicated Mixtures

We have no trustworthy examples of the behavior of complicated mineral mixtures in eutectic relations under known formation conditions, but a brief consideration of the subject from the theoretical standpoint indicates the probability of certain complications in the consideration of two or three component eutectics in the presence of other soluble minerals. With but two pure minerals present, the eutectic appears as the intersection of two curves, and is a point (B, figure 7)—a single mixture in definite proportions. With another mineral present which is soluble in all proportions in both, the eutectic point (with its two minerals separating side by side) becomes a line along which the concentration varies, and (presumably) the structure also. If a fourth mineral is added, its geometrical representation in three-dimensional space becomes difficult, but it is easy to see that the system must have received another degree of freedom, and that the eutectic (still considered as between the two original minerals, but in the presence of two others) is now freely variable in its own composition. In a four-component system, we may have two minerals separating side by side over a range of concentrations and temperatures. It is, of course, conceivable that this divariant system might look more or less like the so-called (!) eutectic structure, though it might separate from very different initial concentrations. To ascribe particular structures to the influence of two or three component eutectics in rocks which contain other minerals, and which very probably contained more volatile constituents during formation, is therefore a somewhat hazardous undertaking at the present stage of development of the subject.

An Illustration of a cooling Mixture

Suppose we now examine the behavior of a cooling mineral mixture in a simple actual case.[24] If we take the mixture of calcium and magne-

[24] We have encountered no actual case in which no undercooling occurs; all the temperatures here given were, therefore, determined from melting material which is simply the converse case of the one here described. The description of the order of phenomena is somewhat clearer in this form.

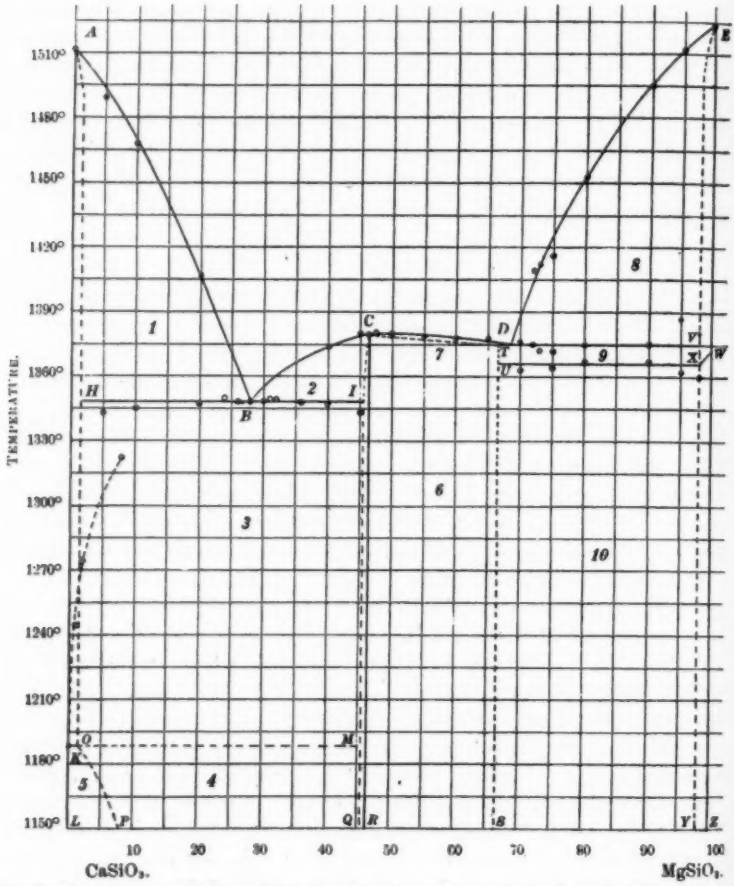

FIGURE 7.—*Diagram showing the Relation between Calcium Metasilicate and Magnesium Metasilicate when mixed in any Proportion at any Temperature*

Principal Areas of the Diagram

1. AHB α-$CaSiO_3$ + liquid.
2. BIC Diopside + liquid.
3. HIMO α-$CaSiO_3$ + diopside.
4. OPQM β-$CaSiO_3$ + diopside.
5. KOPL Mix-crystals diopside in β-$CaSiO_3$.
6. CTUSR Mix-crystals of $MgSiO_3$ in diopside.
7. CDT Mix-crystals of varying composition + liquid.
8. DEV α-$MgSiO_3$ + liquid.
9. TUXV α-$MgSiO_3$ + diopside mix-crystals.
10. UXYS β-$MgSiO_3$ + diopside mix-crystals.

sium metasilicate mentioned above, and suppose it to have been melted and thoroughly mixed in the proportion of 80 parts $CaSiO_3$ to 20 parts $MgSiO_3$ (figure 7), and then to be gradually cooled preparatory to observing solidification, we shall encounter not one change of state only during the cooling, but three, each of which is definitely characterized by a change in the energy content of the system, and is therefore a physical discontinuity which completely defines the stability of the substance with respect to temperature, provided only we are in position to make an intelligent interpretation of what occurs. To follow this case, the first release of heat on cooling appears at about 1405 degrees, and represents the temperature at which, if no disturbance of the equilibrium takes place, the excess of calcium metasilicate begins to crystallize out. At about 1350 degrees, calcium metasilicate and diopside will crystallize in eutectic mixture with a second release of heat. From this point downward all of the solution has become a solid mixture of these two ingredients, but at about 1190 degrees, if the mechanical forces which oppose any rearrangement of the molecules be not too great, the calcium metasilicate will go over from the crystal form known as pseudo-wollastonite into true wollastonite with a heat change similar to the two preceding, but smaller in magnitude. The melting of this mixture is exactly the reverse process. If the original mixture had contained only 10 per cent of magnesium metasilicate instead of 20 per cent, all the other conditions remaining the same, crystallization would have begun when the solution reached a temperature of 1465 degrees instead of 1410 degrees, the other two changes following as before. With a still smaller percentage of magnesium metasilicate, the first crystals might have appeared as high as 1500 degrees.

The same two ingredients in the proportions $CaSiO_3$ 47, $MgSiO_3$ 53, would have given but a single change of state (the melting or crystallization of pure diopside), while with $CaSiO_3$ 20, $MgSiO_3$ 80, three changes of state would have occurred as in the first instance, except that the excess component which first appears is now α-magnesium silicate instead of the corresponding lime compound, and the inversion which follows is an enantriopic change in the magnesium silicate.

If the above explanation has been clear, it offers an illustration not only of the crystallization of an excess component, but of the eutectic and a subsequent inversion of one of the components of the eutectic. It illustrates also the continued lowering of the melting temperature of one mineral ($CaSiO_3$) through the addition of increasing quantities of another mineral. This concrete illustration is quite typical of the general case, in which the solution of one silicate in another will lower its melting

point, not by one degree or two, but by 10, 20, 50, or 100 degrees, oftentimes according to the character of the added mineral and the amount of it which is present.

The adjoining figure (figure 7) representing the complete determination of mixtures of calcium and magnesium metasilicates contains several illustrations of all the features described.

Adding a third mineral to the first two will not only still further lower the melting temperatures, but will increase the number of phases and eutectics to the point of seriously complicating their interpretation. To disentangle such a group of phenomena from the appearance of the mixture in the furnace while the changes are going on is quite out of the question.

Incomplete Reactions

In concluding the discussion of those minerals whose properties are difficult to establish because of their failure to respond promptly to changes of pressure or temperature, attention should be directed to the extreme case, which also is by no means uncommon. If nature has succeeded in crystallizing certain of her most common minerals only through the presence of volatile ingredients, and sometimes not at all (the Obsidian Cliffs) when none are present, she has quite as often paused in the transition from less stable to more stable solid forms. Even qualitative experiments with the blowpipe have developed a great number of cases of instability due to incomplete reaction. One has been worked out in detail in this laboratory. Two forms of magnesium metasilicate have been found which are more stable than enstatite; one has been identified in the Bishopville meteorite, which is supposed to have encountered a very high temperature; the other has never been found in nature. Even before this relation was established, Vogt advanced the hypothesis with a considerable degree of positiveness that all the natural amphiboles represent unstable forms or the incomplete development of the corresponding pyroxenes, and there are many cases of this kind. The minerals which have segregated from the magma have come out step by step in one combination or another, and in one crystal form or another, in obedience to a definite system of transformations, following definite relations of pressure, temperature, and composition. Such a series of transformations may have reached complete stability at the temperature and pressure now prevailing, or it may not. There is a time factor which obtains in all reactions, and it by no means follows, as we have frequently seen, that because geologic time is reckoned in large intervals, sufficient opportunity has been given for all mineral reactions to become complete. If a mineral appears in excess in

a cooling solution, and may take a stable or an unstable form, the unstable form will appear first,[25] and may go over into the stable form afterward, or may not, according as favorable conditions happen to occur or not. Enstatite is a familiar case of an unstable mineral which has probably never attained its stable form except in the laboratory.

The Application of Van't Hoff's Law—Vogt

The most comprehensive attempt to apply these principles of solutions to the crystallization of igneous rock magmas is unquestionably that of Professor Vogt, of Christiania. He employed for the purpose a series of artificial slags out of which he was able to crystallize most of the ordinary constituents of the basic igneous rocks, and the results of his experiments are in reasonably close analogy to the crystallization of these rocks except for the volatile constituents, and the absence of these is probably a sufficient explanation of the absence of those minerals which are missed from his series, such, for example, as the alkali feldspars, hornblende, muscovite, and probably quartz. Vogt's investigations were along two principal lines. He sought to ascertain what mineral first crystallized from a solution of given composition, and, second, whether the lowering of the melting point through the addition of one component to another in such a magma follows the Van't Hoff-Raoult law.[26] In pursuing the first inquiry, he was able to establish for most cases that the first mineral to separate is the mineral in excess of the eutectic proportion. This is the more noteworthy in view of the fact that Vogt made no special effort to obtain or study pure minerals, and his results are therefore wholly qualitative in character. The second investigation, Part II of Vogt's principal memoir, did not result in equally definite conclusions. He was able to establish the fact that the addition of one mineral to another or to a definite mixture of minerals, in general lowers the melting point, as we have just explained, but he was not able to offer satisfactory quantitative evidence of the amount of this lowering. This may have been due to two difficulties:

[25] The demonstration is given in Ostwald, loc. cit.

[26] J. H. Van't Hoff: Die Rolle des osmotischen Druckes in der Analogie zwischen Lösungen und Gasen. Zeitschr. f. phys. Chem., 1, 1887, pp. 481—.

J. H. L. Vogt, Der Silikatschmelzlösungen, II, 1904, p. 128. Vogt applies the law in this form:

$$t = \frac{m}{M} \cdot \frac{.02\ T^2}{q}$$

T = absolute temperature of the melting.
t = amount of lowering of melting temperature.
q = latent heat of fusion per gram of solvent.
M = molecular weight of the dissolved substance.
m = concentration.

(1) the effect of the foreign minerals known to be present which directly affected the relation which he sought to establish; (2) the somewhat unsatisfactory character of his temperature and calorimetric measurements. Then, too, the Van't Hoff-Raoult relation owes its derivation to the gas laws, and has been found to hold by analogy for aqueous solutions up to concentrations of 1 per cent, but not for any others. It is therefore a serious question whether the analogy can be strained to cover the most concentrated and complicated silicate solutions as Vogt has tried to make it. Furthermore, none of the constants of Van't Hoff's law except the temperature have been determined for definite silicates. For these reasons the determinations of the eutectic percentages of orthoclase and quartz, of anorthite and diopside, of diopside and enstatite, and so on through a considerable list, are not satisfactory. Except for the quantitative relations, for which he hardly possessed adequate data, Vogt has shown in a most comprehensive and suggestive way that the laws of solution apply directly and very generally to silicate solutions, and that the relations of the minerals in the rocks can be determined if the necessary measurements can be made at the temperatures where the formations occur.

Summary and Conclusion—The geologic Thermometer

Briefly reviewed, the laboratory situation, then, is this: True characteristics of mineral types can not generally be obtained from natural specimens containing the usual quantity of foreign admixtures. If we are to triangulate our field by the use of mineral types, which would appear to be necessary in whatever light their relations be viewed, we must, therefore, use pure minerals for these types. It has been sufficiently demonstrated that such pure types can be prepared, and with them all the necessary physical characteristics determined. In the discussion of mineral melting temperatures, we differentiate two classes of minerals: (1) those which can be depended on to give a definite point of change of state independent of the experimental conditions which are provided (that is, which remain in equilibrium throughout), and (2) a group of minerals in which the molecular deorientation corresponding to the change of state is so much hindered by viscosity or similar retarding influences that there exists no particular temperature at which the change will be found to begin, and no particular range of temperature or time interval within which it will proceed to completion. This is a property of this group of substances, and not a limitation of the methods of examining them. Theoretically there is a temperature limit up to which the solid is stable, and above which melting will begin and proceed indefinitely, if sufficient time is given. Conversely, there is a theoretical upper limit to the beginning

of crystallization. Practically the beginning may be delayed and the rate variable without limitation, and the assignment of boundaries to the melting interval be therefore wholly arbitrary. The manner of participation of such minerals in rock formation, either in nature or in the laboratory is usually dependent upon the presence of certain volatile ingredients (mineralizers?) which, even when present in very small quantities, appear to increase the molecular mobility of the mineral to an extraordinary degree, and thus to enhance its activity greatly. In dealing with minerals of this class, the duty of the laboratory is clearly to follow nature by replacing those volatile ingredients of which insignificant traces are still found in most of the mineral deposits, and to establish with equal care the progress of the reactions in their presence.

What has been said of the melting temperature is true in somewhat less degree of the inversions of solid crystalline minerals from one crystal form to another. It happens that the energy required for such transformations is relatively small compared with that required for melting, and so but few such have hitherto been discovered. The application of methods of great precision in the Geophysical Laboratory has developed a number of these where none were suspected before, and therefore greatly enriched our knowledge of the development of individual minerals at relatively low temperatures. Furthermore, many of these transformations occur so punctually at a characteristic temperature as to serve an unexpectedly useful purpose in the establishment of a system of geologic thermometry.

In a number of recent publications in this laboratory, the behavior of quartz has been cited in illustration of the facility with which geologic temperatures can be established through the agency of such transformations. We are now able to differentiate sharply, for example, between quartz formed above 575 degrees and quartz formed below that temperature; and this, in turn, is radically different from the cristoballite, which forms above 800 degrees. In each of these cases unmistakable records have been left on the minerals themselves of the transformation through which they have passed. Several other such inversion temperatures are known for the minerals already studied, and together they form the beginning of an accurate system of temperatures of which immediate and extensive application can be made in the study of natural rocks.

The problem of building up a geologic thermometer is a straightforward one. We must select those substances which respond promptly to changes of temperature, and determine for these the temperatures of their changes of state. It may happen that a given mineral will offer but a single reference point of this character, or, like quartz, a single mineral

may yield four or more sharp physical changes which recur promptly at the appropriate temperatures. In our experience it has happened that inversions, or changes of crystal form in the solid state, occur more promptly and are often more definitely recognizable than the melting temperature, even though the quantity of energy involved in the inversion is considerably less. Furthermore, inversion temperatures are much less dependent upon volatile constituents. Pure quartz, for example, melts very slowly over a long range of temperature. Its crystallization does not occur under laboratory conditions without the assistance of volatile substances. On the other hand, its inversion at 575 degrees takes place sharply under the thermal microscope, with never a delay greater than .5 degree, either on the heating or the cooling curve. Wherever such transformations, which are known to occur promptly at a particular temperature, leave an intelligible record behind, we have an absolute bench-mark in geologic thermometry, and the establishment of a sufficient number of such will place in the hands of the geologist a temperature scale of universal application. Nearly every mineral studied yields one or more such points, and hardly more than a beginning has yet been made in this field.

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BEARING OF THE TERTIARY MOUNTAIN BELT ON THE ORIGIN OF THE EARTH'S PLAN[1]

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INTRODUCTION

One of the grandest facts which the science of geology has established up to the present time is the existence of a great world-belt, or girdle, of Tertiary fold-mountains almost encircling the earth. This mountain

[1] Manuscript received by the Secretary of the Society October 4, 1909.

belt comprises the entire Pacific coast of the two Americas and of Asia and extends westward along the southern border of Asia and Europe to the Atlantic coast in Spain and Morocco. In the Malay archipelago the belt branches to the eastward and sends an arm around the northern and eastern sides of Australia, but the mountain ranges of this branch are mostly submerged, and are now represented only by island chains, including New Guinea, New Caledonia, New Zealand, and many smaller islands. Excepting the gap between New Zealand and southern Chile, the Pacific Ocean is completely surrounded by the Tertiary mountain belt. This fact rests upon a sound basis of observations, and is one of the most comprehensive and deeply significant things which mankind has yet learned with certainty concerning the development of the earth. The Tertiary mountain belt is a large element in the earth's plan, and a satisfactory explanation of its origin is a desideratum of the first order.

Under the various forms of the contraction hypothesis of mountain making, this mountain belt has been ascribed to horizontal thrust movements directed from the ocean toward the land. The cause assigned is subsidence of the earth segment which underlies the ocean—a consequence following, according to some, upon secular cooling and contraction, and this has been the dominant view; but upon planetesimal settling and solidification, with some cooling and contraction, according to others, and upon original differences of density, with molecular changes and expansion, producing deep-seated movements from the oceans toward the continents, according to still others.

The remarkable relation of the Tertiary mountain belt to the Pacific Ocean is the thing which is usually most emphasized in discussing the origin of these mountains, and principles based on the supposed meaning of this relation have been applied to other mountains as well, for it is claimed that the relation indicates an oceanic cause which is general in its application. But the fact should not be overlooked that this same mountain belt forms an important part of the periphery of all the continents excepting Africa, and since the belt as a whole falls naturally into divisions or parts corresponding to the several continents to which it has peripheral relations, the question arises as to whether these parts may not be causally related to their associated continents rather than to anything in the oceanic areas.

The several continental divisions of the belt show characters which seem to correspond roughly to continental magnitudes. For example, Asia, or, more accurately, Eurasia, with small parts of northern Africa and Alaska, stand together structurally as one continental unit and constitute by far the greatest of the continental bodies. Asia proper is the

main, central part of this vast crustal sheet, but even this part alone is considerably greater than any other continent. The peripheral mountain ranges of Asia show prominently certain forms—chiefly curved, arc shapes—which are scarcely recognizable or are generally inconspicuous in the peripheral ranges of the other continents. The mountain ranges and plateaus of Asia are greater in every way than those of other continents—greater in areal extent, in height, mass, and breadth of plan. It is a question to be determined whether all these distinguishing attributes are not due mainly to the fact that Asia, considered as a dynamic unit of the earth's crust in the process of diastrophism or mountain making, is larger—greater in extent and mass—than any other similar unit.

If a crust-flake, like that which we call Asia, should move a given distance as a unit, it would certainly produce greater results in crustal deformation than would arise from the movement of a flake having only one-tenth of its magnitude, and the difference in the results would be still greater if the larger flake moved several times farther than the smaller one. The products might differ not merely in magnitude, but to some extent in kind also. Australia is a continent of the same type as Asia, but it is relatively feeble in its development; its peripheral ranges are relatively weak. They are mostly submerged, and, so far as known, they show none of the peculiar forms which characterize the great peripheral belt of Asia.

Figure 1 shows the location of the Tertiary mountain belt. Later researches seem to suggest some slight additions in eastern Asia and in the western and southwestern Pacific, but these are welcome changes and are not discordant.

The mere existence of this vast mountain zone encircling the earth is of itself a remarkable thing, but its significance is prodigiously increased when the distribution and age of these mountains are taken into account and when some of their structural or tectonic relations and characteristics are analyzed. The entire belt is essentially of one age, all made or largely augmented within the limits of one definable and relatively short and recent period of geological time. Moreover, this period of diastrophism is the last or most recent of the great mountain-making epochs—the nearest to us in time. For this reason partly the mountains of this belt are all relatively young—so new that there has not yet been time for them to be destroyed or very greatly reduced by erosion. This is one of the chief reasons for their great height in many places and for the relative youthfulness of their physiographic expression. It is also a reason for the relatively unmodified state of the tectonic lines which they mark, for these lines have not been disturbed or disarranged by any im-

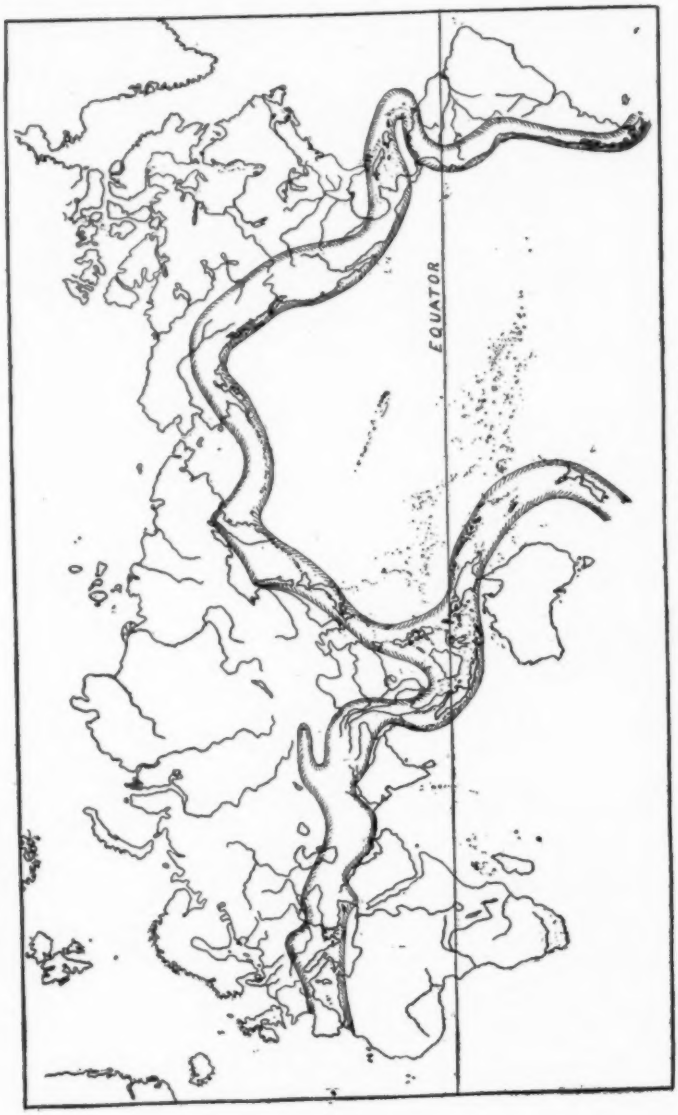


FIGURE 1.—*The World on Mercator's Projection*
Showing the distribution of the Tertiary mountain belt. (From the Berghaus Physical Atlas, 1892.)

portant mountain making of later date. They retain substantially unchanged the forms which Tertiary diastrophism gave them. Hence, whatever characteristics or peculiarities of form or plan mountain-making forces are wont to impress upon their products, these ought surely to be discernible in a mountain system which is at once the newest and most extensive upon the earth. Indeed, the Tertiary mountain belt stands out so prominently as the product of a recent, distinct, and apparently completed epoch of mountain making that within certain limits it can be studied by itself, without much reference to older mountain-making epochs.

It is admitted by all that the mountains of the great Tertiary belt, like the older ranges of fold-mountains, were produced chiefly by compressive forces acting in a horizontal direction, and that the total amount of compression involved is equivalent to many miles of horizontal movement of the earth's crust. What was the nature of those movements? In what direction did the crust move in producing the Tertiary mountains of Asia—from the ocean toward the land or from the land toward the ocean? This is the crucial point.

There is a considerable body of facts which bear strongly upon this question; a few of the more important are briefly set forth below in a discussion of the origin of the Tertiary mountains, and the conclusions thus reached seem to throw some new light on the origin of the earth's plan.

Suess' Interpretation of the Plan of Asia

In his great work, "The Face of the Earth," the eminent Austrian geologist, Eduard Suess, has devoted much space to the consideration of the continent of Asia. As a result of his extensive studies, Suess reached the conclusion that, in consequence of the cooling and shrinkage of the earth as a whole, all parts of the crust have settled or sunken down toward the center, the ocean basins most and the high continental areas least. A few quotations will show the general character of his conclusions. After noting the contrast between the outlines of the ocean basins and the structure of the continents, Suess remarks that "*these ocean basins are areas of subsidence*"[2] (II, 536). "As soon as we recognize the ocean basins as sunken areas, the continents assume the character of horsts" (II, 537). "*The crust of the earth gives way and falls in; the sea follows it.* But while the subsidences of the crust are local events, the sub-

[2] Unless otherwise stated, all quotations from Suess are from "The Face of the Earth," authorized English translation by Sollas. Reference is made to volume and page. All of the italics in the quotations belong to Suess. Four parts of "The Face of the Earth," in three volumes, have been published at the time of this writing.

Showing the distribution of the Tertiary mountain belt. (From the Berghaus Physical Atlas, 1892.)

sidence of the sea extends over the whole submerged surface of the planet. It brings about a general negative movement" (II, 537-538). All changes affecting the height of the sea at one time over the whole globe Suess designates as "*eustatic movements*" (II, 538). "*The formation of the sea basins produces spasmodic eustatic negative movements*" (II, 538). "*The formation of sediments causes a continuous, eustatic positive movement of the strand-line*" (II, 543). In other words, a sinking of the sea floor at any place lets the ocean down from its shores all over the earth, while the piling up of sediments in any part gradually raises the level of the whole ocean. In this way Suess accounts for some, but not all, of the ancient strand-lines now found above ocean level.

Referring to Asia and Eurasia, Suess defines their structure and growth in different words at different times, according to the various points of view from which he contemplates the subject. "Asia consists of an obstructive fragment of Indo-Africa—the peninsula of India—. . . and of a great piece of the earth's crust folded to the south. The folds, however, are interrupted and separated by platforms which lie between them like rigid blocks, although in the platforms themselves we may also recognize the traces of much older folding in the same direction" (II, 195). "*The whole southern border of Eurasia advances in a series of great folds toward Indo-Africa;* these folds lie side by side in closely syntactic arcs, and for long distances they are overthrust to the south against the Indo-African tableland" (I, 596).

Among the several continents Asia seems to afford the most perfect expression of Suess' idea. He describes the ancient "vertex," or central land of Siberia around which the rest of Asia, especially to the east, south, and west, appears to be arranged in roughly concentric zones. This relation finds its most prominent expression in the trend lines of the mountain ranges. The vertex occupies a large region lying chiefly north, northeast, and northwest of Irkutsk, in Siberia. The vertex itself is very old, being capped by horizontally bedded pre-Cambrian strata which appear to have suffered no notable disturbance since their deposition. The surrounding lands are disposed mostly in bands or zones of rocks of younger age, the nearest to the vertex being of Paleozoic age, followed farther out by Mesozoic, and lastly by Tertiary. Thus, going southward from the vertex, the mountain ranges are composed of successively younger strata folded and faulted by horizontal compression, until along the margin of the continent the peripheral ranges belong to the great Tertiary belt. Each belt of folded strata was subjected to horizontal compression in such a way as to make a mountain range usually in the form of an arc opening northward or toward the vertex, and roughly concentric with it.

That he does not stand alone in his understanding of the structure and relations of the mountains of Asia, Suess makes clear in the following passage:

"The uniform structure of the Asiatic mountains has been recognized by all the most eminent authorities on this part of the world, and has been variously described according to the point of view of each observer. In Siberia, Semenow speaks of a succession of terraces; the Gobi, together with the Khingan, forms the highest step; the country of the Amur, with Sikhota-Alin, the second; the sea, with its island arcs, the third. In China, Richtnofen was impressed with the idea that the whole country sinks in great flexures to the Pacific Ocean. In Japan, Naumann compares Asia to an elevated domelike protuberance surrounded by peripheral fractures. As one stands in front of the overfolded chains of the Himalaya, says Griesbach, there seems to have been a movement of the whole mass of Asia toward the south" (III, 7).

From the Philippines to Alaska the whole front of Asia is adorned with a wonderful series of island arcs which have been likened to festoons hanging from the continental border. Concerning the origin of these, Suess says:

"Thus the east Asiatic coast does not resemble a series of independent ranges advancing toward the sea, but rather a *stupendous virgation extending over the whole breadth of Eurasia*, the successive divergence of the same folded systems which, closely crowded together in the interior of the continents, form the great and lofty highlands. In this divergence each of the great branches shows near its extremity—that is, toward the ocean—a tendency to recurve to the north, and thus arise the island festoons of east Asia" (II, 195-196).

In a general survey of the island arcs, Suess mentions the following: the arc of the Liu-kiu Islands, of South Japan, of North Japan, of central Yezo and Sakhalin, of the Kurile Islands, and a fragment of arc in central and western Kamchatka. To these must be added the most perfect arc of all, the arc of the Aleutian Islands, which Suess, however, regards as independent. All these island arcs are, of course, mountain ranges submerged beneath the ocean.

Westward from the Philippines the peripheral mountain arcs continue, and are thus enumerated by Suess:

"Five great arcs turned toward the south align themselves one after the other across the continent; these are the Malay arc, the arc of the Himalaya, the shattered outer arc of the Hindu Kush, the Iranian, and the Dinaro-Tauric arc. To these must be added still another—that which, distinguished by somewhat different characters, surrounds the western Mediterranean" (I, 505-506).

Referring to the southern part of Eurasia, which comprises the great

peripheral mountain belt with which we are here most concerned, Suess says:

"A great part of this folding is of recent age, or has been continued into very recent times; it is not certain that the movement has ended" (I, 597).

And in the summary of his chapter on the relations of the Alps to the mountains of Asia, he says:

"Thus we see that since the Middle Tertiary period, and up to still more recent times, important tangential movements have taken place, and have thrown into folds a sea-bottom which extended through the middle of Europe and Asia" (I, 507).

The ranges here referred to were formed out of sediments laid down in the ancient greater Mediterranean, which Suess calls the Tethys, and of which the modern Mediterranean Sea is only a remnant.

Thus, in brief, Suess finds Eurasia to be a great unit of continental growth which has advanced to its present state by well defined steps from early, smaller beginnings in the far north. First, there was the ancient "vertex" in Siberia. This is an ancient plain, marking the planed-down surface of still more ancient rocks, which, through all the steps of continental development since pre-Cambrian times, have remained remarkably stable and free from disturbance. This plain has not been folded by any of the Paleozoic or later folding movements which have brought into being the great mountain ranges that run in concentric lines around its southeastern, southern, and southwestern sides. Through very long periods of relative quiet, sediments accumulated in the border of the sea surrounding the vertex. Then in relatively short periods of diastrophism or crustal deformation these sediments were squeezed and thrust horizontally in southerly directions—that is, toward the sea—and folded into the mountain ranges. This cycle of continental growth was repeated three times, and produced successively in three periods of folding the three principal mountain systems which characterize Eurasia. In the beginning there were several smaller separate continental elements, and they were not welded together into the one great continental unit of Eurasia until the last or Tertiary folding period. It was this last and by far the greatest of the mountain-making periods since pre-Paleozoic times that brought Eurasia to its present state.

Figure 2 shows the trend lines of the Tertiary fold-mountains of Eurasia. The older lines are omitted. The shaded parts, comprising the peninsulas of India and Arabia and part of Africa, represent the obstructive fragments of the ancient Indo-African continent against which the southward folding of the western half of Eurasia was thrust.

Shows also the irregular trends of the Tertiary ranges of fold-mountains which form the peripheral system. Island arcs of eastern Asia, the Ladrone arc, the Tuscarora and part of the Aleutian frontal deep, and the meeting place of the Australian and Asiatic fold-lines in Borneo, Celebes, and Halmahera.

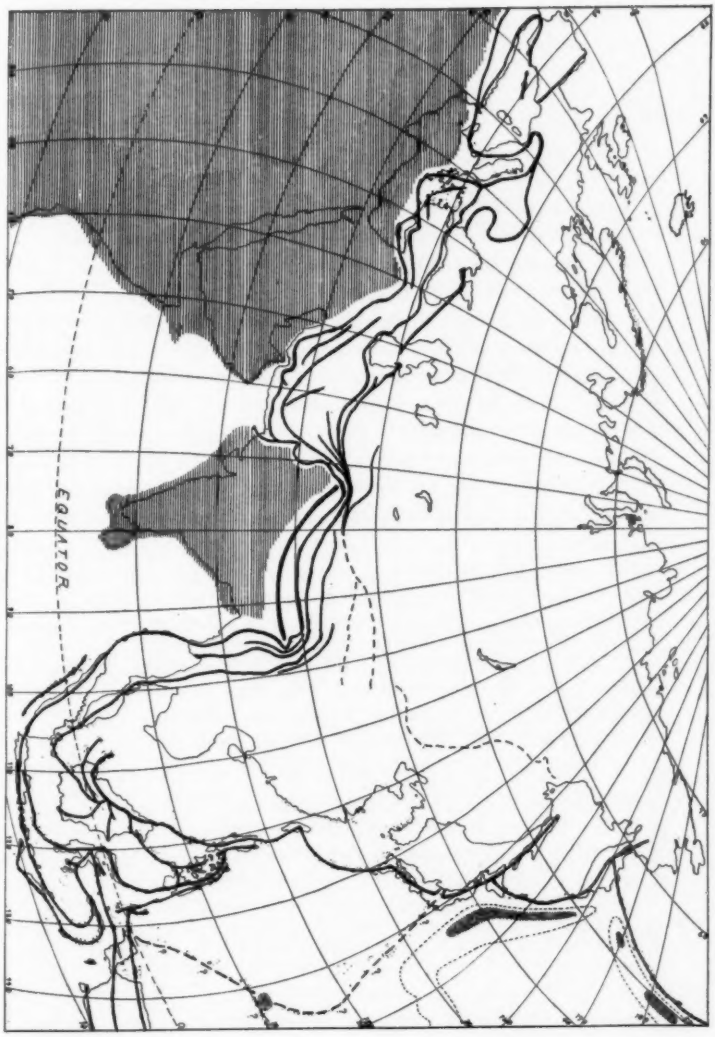


FIGURE 2.—*Eurasia*

Showing the extent and general relations of the Tertiary ranges of fold-mountains which form the peripheral system. Shows also the irregular trends of the European ranges, the Iranian and Malay earth-lobes, the Himalaya re-entrant, the island arcs of eastern Asia, the Ladrone arc, the Tuscarora and part of the Aleutian frontal deeps, and the meeting place of the Australian and Asiatic fold-lines in Borneo, Celebes, and Halmahera.

The trend-lines of the mountain ranges as shown on this map are, of course, somewhat distorted, especially in eastern Asia and in Europe, but they are much less distorted than when represented on Mercator's projection, and the proportions of the parts of Eurasia are more nearly true. The dotted areas east of Japan and south of the Aleutian arc are frontal ocean deeps.

Suess' Method of Interpretation

Suess interprets the structure of Asia chiefly by a study of the horizontal plan of its mountain ranges. These ranges mark the main structural or tectonic lines of the continent, and in the peculiar arcuate forms which characterize them, in the relation of the arc-shaped ranges to one another, in their magnitude, arrangement, and distribution with reference to the continent as a whole, and also in their relative ages and the order of their formation, Suess finds a stronger and more certain light shed upon the structure of Asia and upon the origin of that structure than in all other evidence combined. Suess has reached his grand conclusions as to the direction of crustal creep and horizontal thrust, not by the usual method of studying outcrops and cross-sections, but by a study of the trend-lines of the mountain ranges which were produced by these movements.

This method is unique, but it grew naturally and easily out of the study of the facts observed, and is not a fanciful invention of the imagination. As Suess has said, the arc-shaped ranges of Asia have deeply impressed all the students of that continent. They are the most remarkable and significant thing which the researches in that continent have revealed. While similar investigations have been carried on in all the other continents, this method of interpretation was not developed in any of them, for the evident reason that no other continent has mountain systems which reflect in their plans such simple and unequivocal expressions for the dynamic forces involved in mountain and continent making. But, having been developed in Asia, this method may possibly be applied with success to the interpretation of the other continents.

So strongly significant of crustal movement in one general direction are the lines of the mountain plan of Asia that no geologist who at the outset was free from strong hypothetical preconceptions has failed to agree with Suess in his general interpretation. Not that the other geologists join with Suess in the details of his particular hypothesis of the causes of Asiatic structure, but the general consensus of opinion among European students of Asia agrees with him in his broad conclusion that

the mountain ranges were produced by tangential or horizontal thrusts acting in a general way from northerly to southerly directions.

No other method of studying the products of diastrophism or earth-deformation with the object of learning their causes gives anything like so much promise of final success as this, for none takes hold in such a broad, deep way. Suess' method is adapted to include and comprehend the broadest possible relations and aspects of the phenomena of crustal deformation. The facts dealt with by this method are not a more or less unrelated aggregation of minutiæ, as is too often the case with collections of outcrops and cross-sections; their scope and breadth are commensurate with the extent of the mountain ranges themselves or even with that of whole continents. Indeed, it is one of the objects of the present paper to endeavor to show that the method of Suess is applicable not merely to Asia and Eurasia, but to the whole northern hemisphere and to the southern hemisphere also, and hence to the whole earth; and finally, that the conclusions reached by a broad application of this method point even now very clearly to the general nature of the processes which have produced the earth's plan.

After noting the advances made by Reyer and Bertrand in the methods of study of mountain ranges in transverse section, Suess makes the following remarks on the value of his own synthetic method:

"A study of the mountain chains in transverse section is, however, only one part of our task; we must also investigate them in horizontal projection—that is, in plan. There was a time when every single anticline of the Jura was regarded as an independent axis of elevation; then it became clear that such a collection of parallel anticlines must have a common origin; next it was seen that there is a certain dependence between the Alps and the Jura; finally, the influence of the obstacle presented by the Black Forest was recognized, and it became evident that the Alps and the Jura were only parts of the southernmost, innermost, and most recent of three crescentic systems of folds which have arisen one after the other across central Europe since the close of the Silurian epoch. Thus with our increasing knowledge we are led to the conception of units of a continually ascending order, and the several anticlines of the Jura now appear to us as parts of an organic whole.

"To continue this method of synthesis, to group the folded ranges together in natural units of a still more comprehensive character, and to explain by means of a single simple expression as large a part as possible of the terrestrial folding—such is the task which now awaits the geologist. The *plan of the trend-lines*, written by nature on the face of the earth—this it is which he has to determine" (III, 3).

In the present paper Suess' method of interpretation from the trend-lines of mountain ranges is followed throughout, and is extended to the largest possible units. But his conception of the manner of deformation,

by oceanic depression and tangential thrusts directed toward the oceans from certain northern vertices, or horsts, of relatively restricted area, is not adopted. In its stead an attempt is made in the closing part of this paper to determine in a general way the nature of the causes producing the tangential thrusts, particularly by a study of the largest definable units of deformation and by the groupings which the similar and dissimilar characters of these units suggest in relation to possible causes of deformation. The conclusion reached is one which Suess himself hints at in one of his later works, where he recognizes the possibility of a much broader cause than that which he discusses in the first three volumes of "The Face of the Earth."

Eurasia, the greatest Unit in the Earth's Plan

Significance of the peripheral mountain arcs of Asia

Throughout his whole discussion of the mountain systems of Asia, Suess dwells continually on the significance of the mountain arcs, and especially on those of the peripheral belt. He notes that they all bend to the southward—to the southeast from eastern Asia, to the south from southern Asia, and to the southwest and south from southwestern Asia; that they are overthrust against the northern side of the obstructing tableland of Indo-Africa, and show peculiar forms which arise from adaptation to the form of the obstructing mass; that the island arcs along the east coast, where no obstruction was met, bend out even more perfectly toward the deep depression of the Pacific; that the southward bending of the arcs indicates a connection between the different parts of all this vast region and must have arisen from a common cause, and finally that these characters all contribute to the conclusion that the entire peripheral mountain system of Asia was folded toward the south by horizontal thrust forces acting in that direction. It seems impossible to contemplate a plan of the trend-lines of the peripheral ranges, such as is shown in figure 2, without reaching these conclusions.

The trend-lines show, with two or three exceptions, simple arc-forms bending to the south along the entire front from Asia Minor to Alaska. Their simplicity of form and their relations to one another are such as seem to be attributable only to a single thrusting force acting throughout the whole process of folding substantially in one direction, for there appears to be no evidence of complexity resulting from thrusts in different directions, such as is found in Europe. The arc of the main range of the Himalaya is about 1,500 miles long and is a curve of wonderful simplicity and perfection. It is an almost perfect arc of a circle with a

radius of nearly 1,300 miles, and its curvature is remarkably perfect for a feature of this kind. The island arcs off the east coast are almost as nearly circular in their curvature. But westward from Burma the first range, omitting the Himalaya arc, shows many irregularities that break up the simplicity of its curves.

From Asia Minor to the Philippines the peripheral belt of Tertiary folds is not a single folded line, but consists of two or three lines, sometimes more, lying one behind the other. From the Philippines to Alaska, however, the Tertiary belt is a single fold-line, so far as known. The reason for this difference is not known with certainty. As Suess says, we do not know the character of the platforms upon which lie the seas behind the island arcs; there may be other weaker, lower fold-lines behind the arcs, or the platforms may be composed of ancient, crystalline rocks which moved as "plates" without parallel foldings. In Europe and western Asia the resistance of the Indo-African plateau may have contributed largely to the making of parellel fold-lines, but this would hardly apply to the Malay arc between Burma and the Philippines. Nevertheless, the absence of any obstruction along the east coast would seem to have favored simpler results there.

The peripheral mountain arcs form a continuous frontal fold for the whole southern border of the continent, and are apparently all of one age. Regarded as a product of crustal deformation, this fold appears to be a unit in both extent and time, and it is therefore a unit in dynamic process also. A mighty creeping movement of the earth's crust from the north toward every part of the vast periphery appears to have taken place, and the area of earth-crust involved appears to have been as great or greater than the entire expanse of Asia. This idea is very different from the conception of Suess, who pictures the ancient vertex in Siberia as the center from which all the movements took place, the vertex remaining unmoved, while the crust around it moved away in slightly divergent southerly directions.

THE HIMALAYA RE-ENTRANT AND THE MALAY EARTH-LOBE

The most pronounced departure, however, from a fairly even front for Asia is found in the contrasted forms of the Himalaya re-entrant and the Malay earth-lobe. If we contemplate the plan of the trend-lines, as shown in figure 2 for these two features, it seems apparent that it was the obstructing action of the Indian peninsula which produced the great Himalaya re-entrant. It was the tremendous resistance offered by this fragment of the ancient Gondwana-land which held back the advancing folds to the line of the Himalaya. The effect seen in horizontal plan is

as though India had held back an advancing curtain in a very pronounced way, as indeed it did, for the curtain was the crustal sheet of Asia.

The occurrence of such an obstruction in the way of the southward moving crustal sheet must necessarily have produced some very characteristic effects, and some of these effects could be anticipated with much confidence by the application of well-known principles. It would be expected, for example, that the folds would be most closely pressed together at the most northerly point of the resisting obstacle, where the obstructing effect would be greatest, and that the folds would bend or lap around on either side of the obstructing mass so as to inclose it within a re-entrant angle of the general front. It would be expected also that the vertical component of movement expressed by positive elevation of mountain ranges and plateaus would be greatest against that same point.

All these effects are conspicuously present in the Himalaya re-entrant. The Pamir plateau, the highest on the earth, is close north of the extreme northern point of India, and it is here that the great Hindu-Kush, Kuen-Luen, Altai, and other ranges converge in the Pamir plateau, with the northwest end of the Himalaya range abutting against its southern side. The Himalaya is the most majestic mountain range on the earth and some of its peaks are the highest. Who can doubt that the great height of Gaurisanker and the Pamir are due chiefly to the intensified elevation caused by the obstructing action of the Indian peninsula?

A large area of Asia north of the Himalaya is a high plateau with lofty mountain ranges. At the east the Himalaya range ends abruptly on the Brahmaputra at the great bend, and several lesser mountain ranges wrap themselves around the east end of the Himalaya, some of them changing their trend from due east and west, north of the Himalaya, to northeast and southwest south of the great bend. The simplicity of the relations of all these features is such that the obstructing action of India to the southward advance of the crustal sheet seems the only possible explanation.

From the eastern end of the Himalaya the trend of the peripheral range runs far to the south, curving gradually to the east, and returning northward through the Philippines to Formosa. This great excursion of the peripheral ranges to a point seven hundred miles south of the equator incloses an immense area, including the South China Sea, the Java, Celebes and Sulu seas, the Gulf of Pegu, the islands of Sumatra, Java, Borneo, Celebes, and Philippines, the Malay Peninsula, Burma, Siam, Annam, and part of southern China. This immense area is far too great to be classed as a mere arc along with the other mountain arcs; it is in truth a great earth-lobe, analogous to some of the great ice-lobes

of the Pleistocene ice-sheet of North America. Suess himself suggests this comparison in one of his later works, where he says:

> "One can also recognize a certain resemblance between these curved chains [of Asia] and the course of the moraines, and also the forms of the glacier lobes which Chamberlin draws across the east of the United States."[1]

Its periphery is made up of six or seven lesser mountain arcs joined end to end in a curving course, so as to define the boundaries of the lobe. On its base between the eastern end of the Himalaya and the Island of Formosa this lobe is about 1,700 miles across, and from base to outer extremity it measures about 2,500 miles. It is interesting to note that the Malay earth-lobe presents certain well marked antitheses to the Himalaya re-entrant, and that they are all dynamically normal. Corresponding to a region of relatively free and partly dispersive crustal movements, without obstructing masses, the peripheral ranges of the Malay lobe have comparatively low altitudes, and further, corresponding to an axis of more extensive and perhaps more rapid crustal movement, some of these ranges show unusual volcanic activity. The Java arc in particular is a veritable fire line, and its relatively low mountain basement is almost entirely covered up by volcanic products. Here, then, was the relatively free spreading of a great earth-lobe, the thrust forces dying out in dispersive movements, while in the more severely compressed and overheightened Himalaya and the Pamir we see the effects of crustal movement retarded by a great and effectively resistant obstruction.

There seems to be little doubt that the projection of this remarkable earth-lobe so far to the south on a path lying next east of the Himalaya is in some degree a compensation for the obstructing effect of India. The advance of the crustal sheet in the region north of Indian was so strongly retarded by the Indian mass that the force of the movement against the Himalaya was partly deflected eastward into the Malay lobe. In this way the thrust forces running south along the axis of the Malay lobe were intensified and accelerated, just as a stream of water, meeting an obstruction which fills half the width of its channel, is retarded and slightly raised against the obstruction, only to rush with accelerated velocity through the remaining constricted space. If there had been no obstructing Indian peninsula, it seems likely that these two areas would have been equalized, and that the front line of Asia would have run in a broad curve sweeping from the east end of Arabia to Formosa, without, in all probability, reaching farther south than the tenth degree of north

[1] Bulletin of the Geological Society of America, vol. 11, p. 105.

latitude. In the following passage Suess describes an interesting relation between the Himalaya and Malay arcs:

"We shall show later, with greater detail than we have yet done, that the Himalaya actually terminates on the Brahmaputra. There are chains lying behind the Himalaya joining the meridional chains of Yunnan which pass the end of the Himalaya and are continued in the Malay arc. This arc we have traced through the Banda Islands as far as the coast of New Guinea. But although to the south it passes considerably beyond the equator, yet in a tectonic sense it lies wholly behind the Himalaya, or if we were to number the great folded ranges from the exterior inwards, the Himalaya would receive the number 1 and the Malay arc the number 2" (II, 195).

In the region of Borneo, Celebes, and eastward there are some singular complexities of the trend-lines. These, however, do not belong to Asia alone and will be considered under a separate head.

On the map (figure 2) a broken line is drawn from the west end of New Guinea toward the northeast to the Ladrone Islands, and thence northward to the great fossa in Hondo, Japan. This line appears to mark a submerged mountain range, as shown on bathymetric maps, and probably represents a broadening or uncompleted enlargement of the Malay lobe. This, however, is a matter for further investigation.

Along the south or east front of some of the interior mountain ranges of Asia, Suess, Richthofen, and others have described what they call disjunctive lines or faults with great downthrow to the east, south, or southeast. Along these lines great platforms broke from the high central plateau and settled toward the Pacific. It may be that this movement was secondary or reactionary, and was caused by a too great elevation of the lands of central Asia at the most active stage of folding.

On the west side of India the crustal sheet pressed southward into the gap between the resistant masses of India and Arabia, and formed another distinct feature which we may call the Iranian earth-lobe. It is much smaller than the Malay lobe, but is, nevertheless, distinctly accentuated by the obstructing action of India. The front range of this lobe has two small, sharp re-entrants which appear to correspond to saliences of the obstructing mass. One is on the east side and incloses the plain of Katschi in northeastern Beluchistan; the other is toward the west side, and is marked by the northward bending of the Strait of Ormuz. The Ahkdar Mountain range attains an altitude of more than 6,000 feet back of Muscat, on the east coast of Arabia. This range runs northward into the Strait of Ormuz with decreasing altitude, as though pitching downward to the north under the Iranian earth-lobe. As the Persian Gulf and the Tigris-Euphrates Valley appear to be a sunken fore-land,

so the Strait of Ormuz appears to be a passage over a depressed mountain range.

West of the lower course of the Indus the Kirthar range runs directly south, and then curves southwestward toward the sea at Cape Monze. It appears to continue some distance to the southwest beneath the sea, for the soundings show a deep trough extending back to the northeast along the north side of the submarine line of the range. The position of this range suggests a folded arc curving toward the west through the northern part of the Arabian Sea, but remaining undeveloped. Its trend suggests a continuation in the Ahkdar range of Arabia, which curves back toward the north on the west side, but present knowledge indicates the latter range to be older and to belong to the ancient Indo-African tableland. These features invite further investigation.

THE PERIPHERAL RANGES IN EUROPE

The trend-lines of the Tertiary mountain ranges of Europe are much more complicated than those of Asia. They show a tendency to a dominance of east and west trends, although the Apennine and Dinaric ranges, with northwest-to-southeast trends, form a marked exception, and a considerable part of the Carpathian fold takes the same course. The most remarkable forms, however, are the sharply bent arcs of short radius, the Roumanian arc, the arc of the western Alps, and the arc of the Betic cordillera at the western end of the Mediterranean. All of these present sharply convex fronts toward the west.

In discussing the trend-lines of the European ranges, however, Suess departs somewhat from the methods and principles which he has used so successfully in Asia, and his language in some passages seems to contradict that of others. In Asia, Suess found the direction of overthrusts in the peripheral belt to be invariably toward the south, thus agreeing well with his conclusions on the direction of tangential thrust from the southward convexities of the arcs, and the evident southward compression against India. But one can not be sure upon which kind of evidence he relies the more, overthrusts or direction of arc bending. On the whole, one is inclined to believe that he relies more upon the latter; yet, when he passes over to Europe and discusses the ranges there, he finds a confusion of trend-lines among which, although east-and-west lines are dominant, others are prominent, the Carpathians and the Alps especially showing northward convexities. The three arcs sharply convex to the west Suess attributes to a tangential thrust from the east. The arcs of the Carpathians and the Alps are convex to the north, and from his point of view indicate tangential thrusts in that direction. The arc

of the Apennines is slightly convex to the northeast, while the great curve made by the Apennines and the Atlas of northern Africa is convex to the southeast. The southern range of the Atlas is nearly a straight line, and the Pyrenees, like the Caucasus, are substantially rectilinear.

But, in addition to the northward bending which Suess finds in the Carpathians and the Alps, he finds also that both of these ranges are strongly overthrust toward the north, and that the older ranges also show great overthrusts toward the north or northwest, as in Belgium, Scotland, etcetera. In Scandinavia, however, the greatest of all the overthrusts is toward the east-southeast. On these evidences Suess concludes that the thrust forces in Europe acted mainly toward the north, or contrary to their direction in Asia, and, what is much more important, his language seems to convey the idea that the whole crustal movement in Europe was toward the north. This conclusion he seems to rest on the few cases of northward convexities with associated northward overthrusts.

In some of his broad, generalized statements Suess' words seem to imply clearly enough a general southward tangential movement in Europe the same as in Asia, as, where he says: *"The whole southern border of Eurasia advances in a series of great folds toward Indo-Africa"* (I, 596). Again, in enumerating the continental units he mentions first, *"Indo-Africa*, the greatest tableland of the earth, limited on its northern border, from the point where the Wady Draa discharges into the Atlantic Ocean to the mouth of the Brahmaputra, by the folds of Eurasia advancing to the south" (I, 600). But in discussing the relation of the Alps to the mountains of Asia he says:

> **"We observe as a remarkable phenomenon that from the Caucasus onwards [westward] the tangential movement is not, as in the Asiatic chains, directed to the south, but to the north, and that on the northern border of the Carpathians all the indications appear of an extensive overthrusting on to two forelands of completely different structure, the Russian Platform and the Sudetes" (I, 500).**

In the next paragraph he observes that "it would seem as if in Asia tangential movement or lateral compression had occurred almost exclusively in the direction of the meridians of longitude, but that here [in Europe] it had taken place also in the direction of the parallels of latitude, and it is precisely in this region that the Carpathians are driven out toward the north in so striking a fashion."

In these last two passages, and in others that might be quoted, Suess seems to abandon in part his broader method of interpretation for the older method, which depends mainly upon the study of local structural details, and in doing this he seems to lose sight of a very important dis-

tinction which ought to be kept constantly in view. Overthrusts and local convexities of trend-lines may not accord with the direction of general crustal creep in the same region, and surely it is of the first importance to determine correctly the larger of these facts—the direction of the general crustal creep. This can be done safely only by the broadest possible methods, such as have been used for Asia. Indeed, it is by an extension of the reasoning for Asia to the European field that this question can be most clearly and safely determined.

In Asia the proof that the crustal sheet moved southward as a unit does not rest alone on the existence and relations of the single frontal fold-line, but upon the whole belt of subparallel Tertiary ranges, extending from the Philippines to Asia Minor. The belt as a whole is peripheral in its nature; it is the crumpled margin of the crustal sheet. The belt continues into Europe with about the same width and in the same general relations—as a peripheral belt along the southern margin of the continental sheet.

Suess remarks that there is no natural line of demarkation between Asia and Europe, and Penck happily describes Europe as "peninsular Eurasia."[4] Suess points out the fact that the fold of the Caucasus appears to continue into Europe in the Roumanian arc, the Carpathians, and the Alps, while the Taurus fold continues in the Dinaric range of Europe. In short, the Tertiary fold-belt extends right through from Asia into Europe without any change, except that its simplicity in Asia is replaced by complexity in Europe. The general relations are absolutely identical.

Broad facts like these are of a much higher order of value in determining the direction of general crustal creep than are facts relating to overthrusts and occasional exceptionally directed convexities of trend. The peculiarities and exceptional features in Europe may be explained by other causes, and can not be safely set up as proof of the direction of general crustal movement.

There are at least three causes for irregularity among the trend-lines of the European ranges. The first and most important is the relative smallness of the European crustal sheet, with consequent feebleness of the thrust forces; second, the obstructing and complicating action of block-like masses of the older European ranges, where included within the belt of the Tertiary folds; and, third, the tangential thrust from the east, producing the Roumanian, Alpine, and Betic arcs, as described by Suess.

[4] Science, February 26, 1909, p. 322.

The crustal sheet or "plate," as Suess calls it, lying north and northwest of the European Tertiary ranges, was quite small as compared with that which lay to the north of the Himalaya and the Malay and Iranian earth-lobes, and it was the magnitude or mass of this "plate" which in each case determined the power of the crustal advance and probably also the horizontal distance of the advance. In Asia the Tertiary advance closed and completely overwhelmed the ancient greater Mediterranean Sea, the Tethys of Suess, whereas in Europe the same movement fell far short of this result and closed only a part of the sea, leaving the modern Mediterranean in its present state. It can not be doubted that the southward pressure of the creeping crust was far greater in Asia than in Europe. Where the forward movement of the crust was more vigorous, as in Asia, minor causes tending to interfere and produce irregularities were overwhelmed; but in Europe, where the movement was relatively feeble, the minor forces were favored and the impress of their action remains today.

The thrust from the east, seen in the Roumanian, Alpine, and Betic arcs, appears to have been a deflected force arising probably from the resistance of Indo-Africa to the southward crustal movement in Asia. Although it produced locally a marked effect, it was apparently a relatively feeble force.

On general principles one is inclined to believe that forward overthrusts—that is, moving in the same direction as the general crustal creep, like that at the base of the Himalaya—are likely to be of greater extent than underthrusts which only produce the effect of overthrusts in a backward or opposite direction. Agreeably with this idea, the great overthrust to the east-southeast in Scandinavia has a length of nine degrees in latitude and a known width of over 100 kilometers. This is much more extensive than any of the others in Europe, and the others are all in the opposite direction. It seems probable, therefore, that the Scandinavian case is a true forward overthrust, while the others in Europe are southward or southeastward underthrusts. This, of course, does not take away the appearance of overthrusting in these latter cases, but the tangential force which affected the mass above the thrust plane is in such cases to be regarded as a reflex thrust force, directed backward over the advancing undermass. The upper mass in a case of underthrust might be described as a backward overthrust in distinction from a forward or true overthrust.

The resistance of the African plateau may also have contributed something to the irregular forms in Europe, especially in favoring the persistence of some of the Mediterranean basins, or possibly the making of them. The Atlas Mountain ranges of Africa belong structurally to the

Tertiary folded system of Europe and to the peripheral system of Eurasia.

Since there is no ground for separating Europe from Asia as dynamic units in the Tertiary deformation, they should be taken together as one unit, and the Tertiary ranges of the entire peripheral belt should be regarded as the product of southward creep of the entire crustal sheet of Eurasia. But there is still a little more to be added even to this great unit.

THE ARC OF THE ALEUTIAN ISLANDS

Suess regards the arc of the Aleutian Islands as an independent line, not to be counted with the island arcs of eastern Asia, and he counts it as a distinct and separate element in enumerating the boundaries of the Pacific. This is only partly justified, for this arc is, in fact, distinctly Asiatic in all its characteristics and affinities. The Asiatic character of this arc is strongly shown in its pronounced curvature and its grand sweep. It is, indeed, the most perfect of all the island arcs, and, like the arcs of the Kuriles and north Japan, has a long, narrow ocean deep close in front of it. Its backland belongs about two-thirds to Asia and one-third to North America. The mountain chain which forms the arc rises from the sea as it approaches Alaska, and continues in the same curve to the highest part of the Alaskan Mountains.

While the Aleutian arc is a perfect type of the Asiatic arcs, no such arc occurs in either of the American continents. Probably Suess regarded it as independent, because it is so situated that it could not be related to his Siberian vertex in the way that he finds the other island arcs to be; but this is not believed to be an essential distinction. That part of Asia which forms the backland of the western part of this arc is just as much a part of the great crustal sheet of Eurasia as any other part, and the distinctly Asiatic type of the arc shows that whatever special conditions determined the peculiarities of the other island arcs affected the backland of the Aleutian arc in the same way.

When the Asiatic character of the Aleutian arc is recognized it becomes at once apparent that the crustal sheet of Eurasia is not limited on the east by a line through Bering Strait dividing this arc and its backland, but includes the whole arc and the whole of its backland. Thus, Asiatic character is carried eastward to the heart of the Alaskan Mountains, where the curve of the Aleutian arc meets the Cordilleran ranges of North America in a sharp angle. This meeting point falls near the 148th meridian of west longitude, and when we consider continental boundaries with reference to Tertiary diastrophism, we must include in

the crustal sheet of Eurasia all that part of Alaska which lies west of this meridian.

Thus Eurasia, considered as a crustal unit in the Tertiary movements, includes all of Europe with the Atlas ranges and Canary Islands of northwestern Africa; all of Asia excepting the peninsulas of Arabia and India, and in addition all that part of North America which lies west of the mountain angle of Alaska. It seems certain that all this vast crustal sheet was affected by a horizontal creeping movement in the Tertiary age, that it all moved in a southerly direction substantially as a unit, and that the entire belt of Tertiary fold-mountains which forms its southern periphery was made at that time and by that movement.

FRONTAL OCEANIC DEEPS

The deepest parts of the ocean are found mostly in long, narrow troughs closely parallel to the continental borders, especially that of eastern Asia.[5] The position and relation of these deeps are truly remarkable. The Tuscarora deep, with a depth of more than 5 miles, is the deepest abyss now known. It lies in front of the Kurile Island arc and the arc of north Japan (see figure 2). An even more remarkable deep is that which lies close along the entire front of the great curved arc of the Aleutian Islands and includes the Supan and Maury deeps. This trough is partly shown in figure 2. The principal frontal deeps are shown in figure 7.

Other deeps in the same relation, but smaller, occur along the east side of a submerged escarpment running north from New Zealand. These lie in the northwest part of the more extensive Aldrich deep. There are deeps along the west coast of South America, such as the Bartholomew, Richards, and others. Deeps with circular or irregular outlines occur in other relations not so clearly dependent upon adjacent land-masses. Such are the deeps of the Atlantic and the line of deeps running north from the Aldrich deep through the middle of the Pacific. The Challenger deep is near the junction of the Ladrone and western Caroline Island chains, while the Wharton deep lies partly between Australia and the Malay arc. Deep holes, like the Bartlett and Weber deeps, are characteristic of all the Mediterranean seas.

The Tuscarora deep and that in front of the Aleutian arc seem clearly linked causally with the continental border and the great mountain ranges

[5] See Sir John Murray's "Bathymetrical chart of the oceans." Scottish Geographic Magazine, vol. xv, no. 10, October, 1899; opposite p. 560. Reproduced in Chamberlin and Salisbury's Geology, vol. i, p. 10. The portion covering the Atlantic Ocean is reproduced as plate 4, facing page 217.

which stand adjacent to them. They are of the nature of sunken or depressed forelands, and are apparently due to the stupendous weight and pressure of the adjacent ranges. These arcs are no doubt more or less overthrust upon the ocean floor, and the troughs are probably due in part to elastic yielding and perhaps in part also to plastic flow.

The most strongly marked deeps are close in front of island arcs which represent submerged mountain ranges. Evidently the reason that the Tuscarora and Aleutian deeps remain unfilled today is that the ranges to which they are related have remained submerged, have suffered little or no erosion, and hence have supplied very little sediment. Other similar frontal depressions situated close to continental lands which supplied great quantities of sediment have been partly or wholly filled. Such are the valleys of the Ganges and Indus in India, the Tigris-Euphrates Valley and the Persian Gulf, and also the Adriatic Sea and the Po Valley. Such troughs, growing deeper while sediments are being deposited in them, furnish a possible explanation of certain sedimentary strata whose great thickness and shallow-water character seem to demand subsidence during deposition.

The fact that the greatest deeps, both unfilled and filled, lie close to the front of the peripheral ranges of Eurasia, the greatest of the continental units, adds one more significant group of facts to the great aggregate, showing how much more vigorous were the Tertiary crustal movements there than in any other part of the world; and they join with the other evidences mentioned above, which show a general southward crustal movement for the whole of Eurasia.

The Relation of North America to Eurasia

The mountain knot of Alaska

In his earlier writings, Suess regards North America as a continental unit which moved in harmony with the Tertiary movement of eastern Asia—that is, it was folded toward the Pacific Ocean. Referring to North America, Suess says: "So far as folding is known in this continent, it appears to be everywhere directed to the west" (I, 600).

It may be observed here, however, that the Tertiary folds bordering the Pacific are mainly folded toward the *southwest*, rather than to the west, as Suess states, and that only in the States of Oregon and Washington are they folded to the west.

On the same page Suess observes further that—

"After having described the manner in which the syntactic arcs of the great chains push forward against the north of the Indian Peninsula, we observe that a similar advance of syntactic arcs takes place toward the north of the Pacific Ocean, and that a special tectonic homology exists between that fragment of ancient table-land and this part of the ocean."

From these passages it seems clear that Suess regarded the Cordilleran ranges of British Columbia as pressing toward the southwest to meet the eastern end of the Aleutian arc pressing toward the southeast, and this is apparently the true relation. There was thus a convergence of crustal movements in the mountain knot of Alaska, and it was the conflict of these movements which intensified the mountain making at the angle and gave the mountain knot its greater breadth and height. Figure 3 shows the mountain knot, with part of the Aleutian range which enters it from the southwest and part of the Cordilleran range which enters it from the southeast. The shaded part is 5,000 to 10,000 feet in altitude and the black parts 10,000 feet or more.

The morainic accumulations of the Pleistocene ice-sheet in North America present homologous forms that are very instructive. These are the interlobate moraines which were produced in re-entrant angles of the ice-front, where the fronts of two adjacent great lobes of the ice-sheet came together on converging lines. The mountain knot of Alaska is in a precisely similar sense an interlobate form—a confusion and intensifying of mountain making in the angle between two great earth-lobes, or two crustal sheets moving on converging lines. Just as interlobate moraines are higher, more bulky, and more tumultuous in form than either of the single morainic ridges approaching the interlobate angle, so the mountain knot of Alaska is broader and higher than either of the ranges approaching it. If the Cordilleran ranges of British Columbia had been folded toward the northeast—that is, away from the ocean—there would be no reason for the existence of the mountain knot; and still less if we suppose, further, that the eastern part of the Aleutian range had been folded to the northwest. There would then be diverging movements from the place of the mountain knot—a condition hardly favorable for the formation of such a feature.

American geologists, following H. D. Rogers, Dana, Leconte, Dutton, and others in their interpretation of Appalachian structure, made many years ago, derive the thrusts producing the folding of that range from the direction of the Atlantic Ocean. No doubt this is correct so far as relates to the immediate thrust forces involved—that is, to those which

affected the upper or outer parts of the crustal sheet and produced the Appalachian folds. But these authors make no distinction between the relatively superficial thrust movements and the deeper general creeping movement which involved the whole crustal sheet of the continent, as it was then, and reached downward to or into the zone of rock flowage. The variously directed thrusts which affect the more superficial parts may, according to local conditions, act in any horizontal direction, but usually either in the same direction as the general crustal movement or in the opposite direction. The same idea was later applied to the Rocky Mountains and to the Cordilleran ranges bordering the Pacific, and the thrust forces there were derived from the Pacific depression.

FIGURE 3.—*Alaska*

Showing the mountain knot, part of the Aleutian Island arc, and part of the Cordilleran ranges of British Columbia and Alaska. The arrows show the supposed direction of crustal movements.

It seems certain, however, that the relation of the Cordilleran to the Aleutian range in the mountain knot of Alaska shows that the general crustal movement of North America in the Tertiary age was toward the Pacific, and hence that the great eastward and northeastward overthrusts of the Rocky Mountain region are in reality underthrusts, or reflex overthrusts, directed backward over the southwestward general crustal movement. At these localities the southwestward underthrust of the deeper

parts was the primary movement, and the apparent northeastward overthrust observed in the superficial parts is regarded as secondary and reflex in its nature.

As shown in the two quotations given above from Suess, this was in substance his original interpretation of the part played by North America in the Tertiary mountain making which set the present boundaries to the Pacific. He regarded the crustal sheet of North America as folded southwestward toward the Pacific. Thus, on principles derived from his study of Asia, Suess generalized from Eurasia to North America, and concluded that the latter, like the former, had been folded toward the great ocean.

When Suess arrived at this generalization he had accomplished a great and magnificent result for geology, for he showed that North America, like Eurasia, had been affected in Tertiary times by a crustal movement in a southerly direction—not exactly to the south, but southward with a strong deflection to the west, though not more strongly than was the southward movement in eastern Asia deflected toward the east. In this conclusion Suess had completed in rough outline the Tertiary tectonic history of the northern hemisphere. He had put both of the great northern continents into the same category with reference to Tertiary crustal movements. Regarding each northern continent as a crustal unit, Suess found that Indo-Africa had remained stationary, or at least without tangential movements, but that both Eurasia and North America had been affected by tangential movements in the same general direction—that is, toward the south. It should be noted that in this distribution there is a certain relation to latitude, for Indo-Africa, lying mainly in the tropical regions, remained unmoved, while Eurasia and North America, each extending vast areas far to the north, both of them into the arctic regions, crept away to the south.

Here, then, is a fact as broad as the northern hemisphere. Both of the only two continents whose crustal sheets reached far to the north moved in the Tertiary age from north to south. Such a fact as this may be taken as the concrete expression of a general law of wider scope. Thus, if the forces of Tertiary diastrophism caused the earth's crust to move from north to south in the northern hemisphere, may we not take this distribution of the deforming force to be characteristic and fundamental? Putting it in abstract form, may we not say that the deforming force caused crustal creep from high latitudes toward low latitudes? But if this statement expresses the truth it ought to be as applicable to the southern hemisphere as to the northern, and here we are naturally inclined to turn to the antipodes to see what the evidence is there.

However, before turning to this new field there remains one other important body of facts which bears strongly upon the direction and amount of movement of the North American crustal sheet.

GREENLAND, THE GREAT NORTHERN HORST

If further evidence were needed to make sure that in the Tertiary diastrophism North America moved toward the southwest—toward the Pacific and the Tertiary fold mountains along its border—it may be found in the remarkable relation of Greenland to North America.

Suess remarks that "Greenland is a horst of the first order between two or more sunken areas of different age" (II, 294). But Suess' conclusion was based mainly on different evidence from that offered here.

A map of Greenland and its environs on a large scale shows some remarkable characteristics in the outlines of the channels, straits, and bays on its northern and western sides. These are shown fairly well in figure 4.

Amid such a tangle of irregular straits and channels as separate the islands of the archipelago west of Greenland, it is quite surprising to find a passage so straight and persistent as that which separates Grant Land, Grinnell Land, and Ellesmere Land from the north part of Greenland. It seems like a distinct rift-line, and the question arises as to the direction and amount of movement along it. It is quite different in character from the great rifts or fault lines which Suess supposes to form the sharply cut boundaries of the east and west sides of India, Africa, Madagascar, and Greenland. It is a relatively narrow passage, and one seems driven to the conclusion that the displacement along it was a horizontal movement parallel with the rift. In looking for the direction, it seems impossible to suppose Greenland to have moved toward North America and the land on the west side of the rift relatively in the opposite direction, for in that case the movement would only have made Baffin Bay, Davis Strait, and the Labrador Sea more narrow, and there would be no reason to expect any element of parallelism in their sides. Besides, if Greenland was formerly farther east the straight coast along the north side of Peary Land would not necessarily be related to or determined by the course of the rift; it might be so related or it might not. Further, there is no independent evidence that Greenland has moved at all, but in the mountain knot of Alaska we have independent evidence that North America has moved toward the southwest. Thus we may conclude, at least provisionally, that it was North America that moved away from Greenland, not *vice versa*.

If, then, by reversing the process, North America be in imagination pressed back northeastward to a complete union with Greenland, it is evident that the Labrador Sea, Davis Strait, and Baffin Bay would be

FIGURE 4.—*Greenland and the Arctic Archipelago*

Showing the rift valleys on the northwestern and western sides of Greenland. The arrows show the supposed direction of crustal creep and the lines AA, BB, etcetera, show the supposed distance of movement.

closed and obliterated entirely, and that Grant Land, Grinnell Land, and Ellesmere Land would be thrust northeastward past the north end of Greenland along the line of the rift—that is, along the line of Smith Sound and Kennedy and Robeson channels produced.

It is surprising what relations a few measurements reveal in this region. Suppose Grant Land to have been situated formerly close north of Peary Land, from which it was then separated only by the rift valley. Its position in this relation is sketched in a broken line in figure 4, and shows the east coast of the two regions to form a nearly north-and-south line. If a straight line AA be drawn along the axis of the rift from a point close off the extreme eastern end of Grant Land to another point similarly related to the extreme eastern end of Grand Land *in its assumed former position*, the length of that line is about 330 miles. It is noticeable that the rift valley curves slightly to the south as it runs along the northwest coast of Greenland, and perhaps eastern Grant Land should have been placed a little closer to Peary Land. But if the lines BB, CC, DD, EE, FF, and GG be drawn nearly parallel to AA, it will be found that some of them, especially DD and FF, connecting the opposite shores of Baffin Bay, are of the same length as AA, and that EE and GG are suggestively near the same length. It is certainly significant that the distance across Baffin Bay on lines parallel to AA are so nearly of the same length. Baffin Land, therefore, appears to have been pulled away from Greenland in the same direction as Grant Land, and, what is more significant, it appears to have moved the same distance. If this be true, then we may believe that the bays or inlets marked on opposite sides of the rift valley at the ends of the lines BB and CC were offset by the movement and were formerly exact opposites. Too much importance ought not to be attached to these two lines now, but it is not impossible that the correspondence of these features may some day be proved by geological investigations.

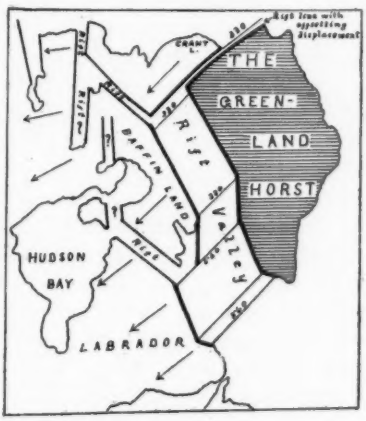


FIGURE 5.—*Diagram of Greenland Rifts*

Shows offsetting rift on northwest side of Greenland, open rifts of two widths on the west side, and several smaller subsidiary rifts farther west. The arrows show the direction of crustal movement, Greenland remaining stationary.

Up to this point some doubt might be entertained as to the supposed value of these relations and measurements. But one further fact of a strongly corroborative character remains to be mentioned. Although Grant Land and Baffin Land moved over 300 miles to the southwest,

they did not move as far as did Labrador and the main body of the continent. The relation of the coast of Labrador to the west coast of the south part of Greenland is truly remarkable. This is shown by the lines LL, MM, NN, and OO. These lines are parallel with the lines DD and FF on Baffin Bay, but they are considerably longer, being each about 560 miles. Thus for a distance of about 450 miles the two shores of the Labrador Sea, although now 560 miles apart in the direction of the rift along the northwest side of Greenland, are almost exactly parallel and the geological age and structure of the rocks, so far as known, are the same. If the crust pulled away evenly, as these facts suggest, one would expect Grant Land and Baffin Land to be separated from the main body of the continent by other narrower rifts. Such rifts may be represented by the long, straight channel of Lancaster Sound, Barrow Strait, Melville Sound, and Banks Strait. A longer, less continuous passage, probably representing a wider rift, follows Hudson Strait, Fox Channel, Gulf of Boothia, and McClintock Channel.

We seem to have here a great irregular rift line along which North America has been torn away from Greenland. One part, extending along the northwest coast of Greenland, is a longitudinal rift, a great fault line with horizontal offsetting displacement. The other part, comprising Baffin Bay, Davis Strait, and the Labrador Sea, is a transverse rift along which one side was pulled horizontally away from the other. The two together are over 2,500 miles long. Then there are also the lesser transverse rifts mentioned above.

Perhaps no one of these measurements or relations taken by itself is of much value, but the assembled group makes a strong case for the pulling away of North America from Greenland. The Labrador coast is high and bold, and the Greenland coast facing it is moderately so. Both coasts have deep water close off shore. The parting of these shores can hardly be more recent than the Tertiary, nor is it easy to believe that it is much older. Even if it was so recent as the Tertiary, it is quite surprising to find the opposite walls of a rift 450 miles long pulled 560 miles apart, and still remaining so strikingly parallel; and this, in spite of all the elements of irregularity which might be expected in such a movement, in spite of the largely accidental relation of the sea surface to the rift walls, and in spite of all the erosion that has taken place since. It seems altogether incredible that such characters should have been preserved from times much older than the Tertiary. Labrador is solidly intact with the main body of the continent, and the rift of the Labrador Sea may therefore be taken tentatively as an approximate measure of the distance of the horizontal crustal movement of North Amrica in the Tertiary diastrophism.

M. L. de Launay, probably following the suggestion of Suess, placed a Tertiary rift along the axis of Baffin Bay, Davis Strait, and the Labrador Sea, and also one on the east side of Greenland and southward through the Atlantic, but he does not appear to have noticed the longitudinal rift on the northwest side and the very significant measurements on the lines parallel with it.[6]

On the remaining sides of Greenland there are no suggestive parallel coasts. But it seems certain that the crustal sheet of Eurasia pulled

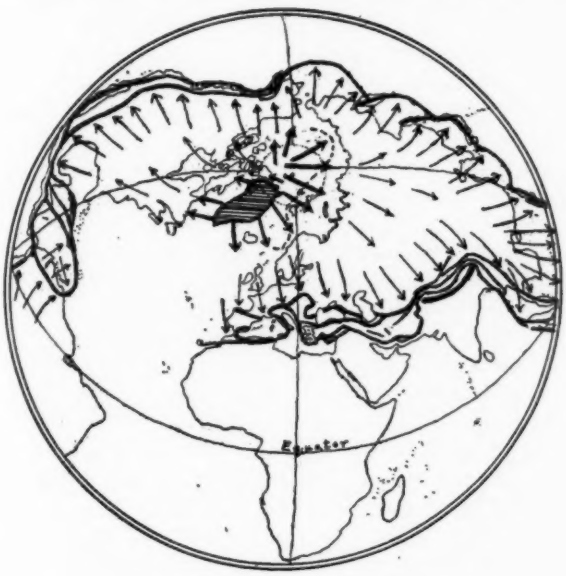


FIGURE 6.—*The Landsphere*

Showing the relation of Greenland to the surrounding continents and to the peripheral mountain ranges of Eurasia and North America. The light arrows show the direction of crustal creep and dispersion. The heavy arrows radiating from Greenland and the pole show roughly by their lengths the relative distances the continents have moved toward lower latitudes. The longest arrows point toward Asia, the shortest toward North America. The broken line north of Asia marks the edge of the continental shelf.

away from the east side of Greenland and from the region of the pole north of Greenland for a distance two or three times as great as the parting between Greenland and Labrador. These relations are roughly sketched in figure 6. It seems probable that the northwest coast of Nor-

[6] La Nature, January 21, 1905. Abstract in English: Literary Digest, March 18, 1905, pp. 396-397.

way was once united to the east coast of Greenland, although they are now over 1,000 miles apart, and the same may have been true of the northwest sides of Scotland and Ireland. There is less reason to think that the rifts on the sides toward Eurasia are wholly of Tertiary age; they may have been made partly at an earlier time, as suggested by Suess.

It may be noted that the greatest breadth of the Arctic Ocean, and probably of the parting from Greenland, is toward Asia, including the Aleutian arc. It is widest toward the island arcs of eastern Asia and toward the Malay earth-lobe and India. In figures 6 and 7 one can scarcely fail to see that the western coast of North America shows only faint, incipient arcs, corresponding to less crustal movement and the relatively narrow rift on the west side of Greenland, while on passing to the east coast of Asia the great island arcs bulge boldly into the depression of the Pacific, corresponding to a much more vigorous crustal movement and to a much wider rift between Greenland and Asia.

It is thus seen that the idea of a general crustal creep from high toward low latitudes in the northern hemisphere is borne out, not alone by the peripheral mountain ranges which fringe the southern border of Eurasia and the southwestern border of North America, but also by a rifting and pulling away of the earth's crust on all sides of Greenland, and that the amount of pulling away is least toward the feebler peripheral ranges and greatest toward the stronger.

The following are the faint peripheral arcs of North America: (1) The Alaskan Island arc, comprising the island chain of southeastern Alaska with Queen Charlotte and Vancouver Islands; (2) the Coast Range arc, extending from the Strait of Juan de Fuca to southern California, and (3) the Mexican arc, extending through Lower California and southern Mexico to the Isthmus of Tehuantepec. These arcs are shown in figure 7, but they are all faint and of slight curvature. They show much better in a map of North America drawn on spherical projection.

Suess has much to say concerning the remarkable correspondence of the Paleozoic and older sediments, and also of the mountain ranges on the two sides of the North Atlantic. He describes "The North Atlantic Continent" at some length (II, 220-255), and shows that it persisted until a very recent time in the earth's history. "We have recognized the existence of two continents, of which fragments only are visible at the present day. The first occupied the position of the north Atlantic Ocean, as is indicated by the nature and distribution of the Paleozoic sediments in Europe and America; Greenland is a remnant of it. This ancient continent is the *Atlantis*" (II, 254). Suess dwells upon the likeness of the Carboniferous sediments as being especially remarkable. The other fragmental continent referred to is Gondwana-land.

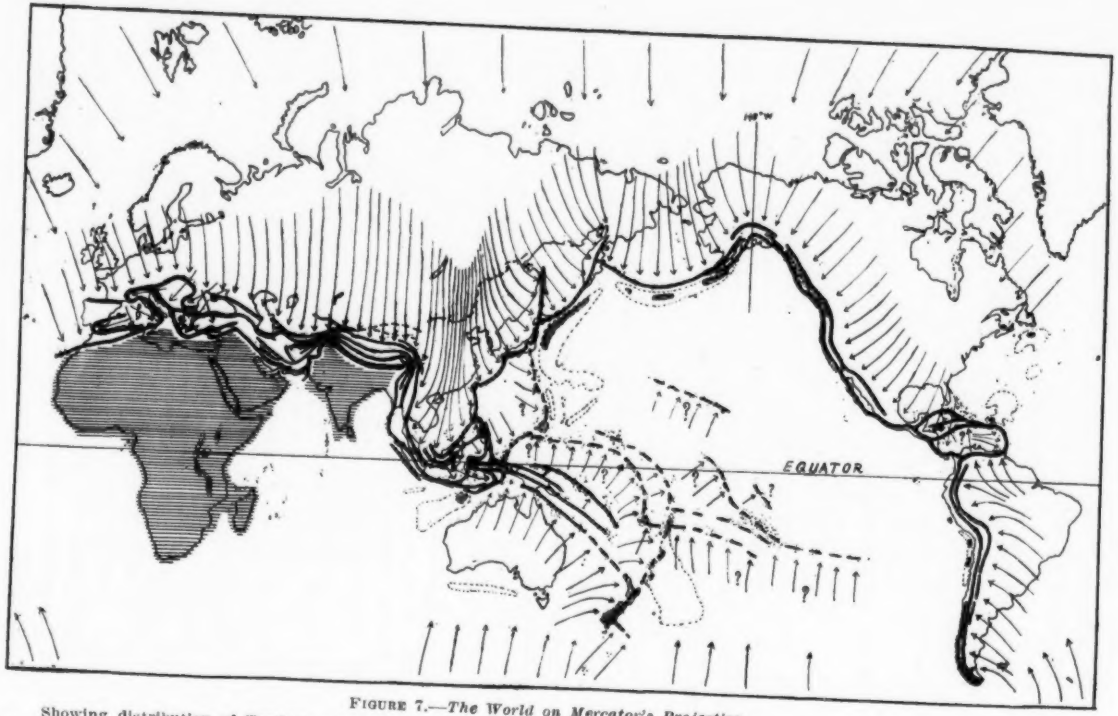


FIGURE 7.—*The World on Mercator's Projection*

Showing distribution of Tertiary mountain ranges in both hemispheres. The arrows indicate roughly the direction and relative amount of crustal movement. The map shows also the positions of the frontal oceanic deeps, the relations of the Tertiary ranges of Europe and western Asia to the Indo-African plateau, and the peculiar plan of the ranges between Asia and Australia and in the Caribbean region.

The recentness of the breaking up of Atlantis, as described by Suess, accords well with the conclusion that the rifts around Greenland are of recent date. The continent was pulled asunder apparently toward the southwest and southeast along a line passing southward from Cape Farewell and dividing around Greenland so as to leave it unmoved.

Penck, after enumerating many similarities between Europe and eastern North America, remarks that these similarities are not superficial.

> "In a very remarkable way these two sides of the Atlantic repeat the same structural features; there is an astonishing symmetry, as Eduard Suess has shown so clearly." "It is very interesting to see how the Appalachian region ends at Newfoundland, forming the projecting eastern corner of North America, and just opposite in south Ireland, in south Wales, in Cornwall, and in Brittany the belt of the old Hercynian Mountains of Europe begins. One seems to be the continuation of the other, and such an excellent geologist as Marcel Bertrand maintained that we have here to deal with the two ends of one very extensive belt of mountains which extended through the North Atlantic Ocean. But we must not forget that the missing link between both ends of these supposed mountain chains is longer than their known extent."[1]

No doubt some portion of each of these mountain chains is now submerged beneath the Atlantic. But it seems probable that a considerable part of the present oceanic interval is due to Tertiary and perhaps to older crustal movements which divided the original chain near Greenland and carried the parts away on divergent lines—to the southeast and the southwest. In a later work, referred to below, Suess again dwells particularly upon the remarkable similarities on the two sides of the Atlantic.

In the southern hemisphere South America appears to have crept away to the northwest and Australia to the northeast, but these two continents are nearly 180 degrees apart. Africa holds a medial position remotely suggesting a relation similar to that of Greenland to North America and Europe, but in reality the similitude fails, because Africa is tropical.

Tertiary crustal Movements in the Southern Hemisphere

AUSTRALIA

Passing by Africa and the Antarctic land, there are only two continents in the southern hemisphere, and they are relatively small. Present knowledge of them is rather meager.

In its continental type Australia resembles Asia. Both are roughly symmetrical in form, so far as relates to Tertiary diastrophism, whereas South America resembles North America, being unilateral or asymmetri-

[1] Both passages from *Science*, February 26, 1909, pp. 322-323.

cal in form. The Tertiary fold lines of Australia and South America may be seen in figure 7. In these foldings the main body of Australia appears to have moved toward the northeast, but the movement was relatively feeble. All of its peripheral ranges are submerged in the ocean and are now represented only by chains of islands. New Guinea, New Zealand, and New Caledonia are islands of large size, but most of the islands are small. Between Australia and the equator there are three or four distinct lines trending mostly southeast to northwest. These lines are nearly straight, excepting that toward the northwest they turn to courses due west. Two or three of these lines run through New Guinea, one apparently extending southeast through New Caledonia. This line does not appear to connect with New Zealand. Another strong line runs through the Admiralty Islands, through New Mecklenburg, the Solomon Islands, New Hebrides, and eastern Loyalty Islands.

The alignment of the smaller islands is not clear in some parts, but one quite distinct line appears to begin north of the equator in the Egoi Islands of the western Caroline group, and, after running eastward 1,500 miles, sweeps in a great curve to the south through the Gilbert and Ellice Islands to the Fiji group, where it meets another line from the east. The greater size and height of the islands of the Fiji group as compared with those of the lines that enter it from the north and east reminds one of the island arcs of eastern Asia, where the points of intersection of the arcs are always higher than the arcs themselves, as in Kamchatka, Hokaido, and Formosa. Another line less clearly defined begins in the Marshall Islands, and appears to curve gradually around through New Zealand, the Phœnix, Tokelau, Samoan, Friendly, and Kermadec Islands. North of New Zealand there appears to be a line of narrow ocean deeps close along the east side of a submarine escarpment running from East Cape, New Zealand, to the Friendly Islands. This escarpment indicates folding from the west on the line of New Zealand produced, and it shows clearly that it is the Marshall-Samoa-Friendly line and not the Caroline-Gilbert-Fiji line which connects toward the south with New Zealand. New Zealand itself seems to owe its pronounced development to the junction of important lines, at least one to the northwest from the north island and one to the southeast from the south island.

The Hawaiian Islands, though so far away, appear to have a distinct affinity for the Australian lines southwest of them, and they appear to be just as distinctly independent of Asia and the Americas. The Washington-Christmas line lies farther south, with the same trend. The Samoa-Society and Fiji-Cook-Austral lines run east-southeast, while the Taumotu and the Marquesas, on lines still farther east, trend to the

southeast. They seem to show some affinity for Australia and at the same time are somewhat independent. Samao, like Fiji, seems to be a point of intersection for lines from the north and the east. These eastern lines seem to show no affinity for South America and very little for Australia, but are perhaps related more closely to a submerged crustal sheet lying to the south.

BORNEO, CELEBES, AND HALMAHERA

The peculiar mountain plans of these three islands have attracted the attention of geologists for more than a century, and the explanation of them has remained a puzzle. Their forms are shown in some detail in figure 8, but their relations to the trend-lines of the peripheral ranges are better shown in figure 2.

It will suffice for the present to point to the fact that they occur just where the advancing folds of Asia and Australia came into conflict (see figure 2). In each of these islands we seem to see a fold belonging to the southeast part of the Malay lobe advancing broadside against the end of one or more Australian folds, and both sets of folds are affected by the encounter. Celebes seems to show the simplest relations. The Malay fold appears to have been retarded and its trend-line indented by the Australian fold, while the Australian appears to be broken and reflected back. The mechanics of these forms, however, are not yet clearly understood.

The plan of Borneo strongly resembles that of Celebes, but it has been elevated so that its platform is above the sea. Halmahera is also a miniature of the same type.

Another point illustrated here is that where the Tertiary belt has several ranges, not only were the back ranges made first, but during the later folding of the front ranges the back ranges were subjected to a movement of elevation without further folding. Borneo is largest and highest, Celebes smaller and lower, and Halmahera still more reduced. We seem to see also a dying out of intensity from Borneo to Halmahera. Is it not significant that these strange forms occur just here at the point of conflict between the Tertiary mountain belts of Asia and Australia?

Suess calls these forms "chiragratic," and places Chalcidyce and Morea of the Dinaro-Tauric arc in the same class (I, 506). The latter are peculiar, but they are not the same as the Malay forms, and were not produced in the same way—that is, by a broadside-to-end conflict of folded ranges.

SOUTH AMERICA

In the present state of knowledge there is not much to say about this continent, for it is decidedly the most abnormal of any that were affected

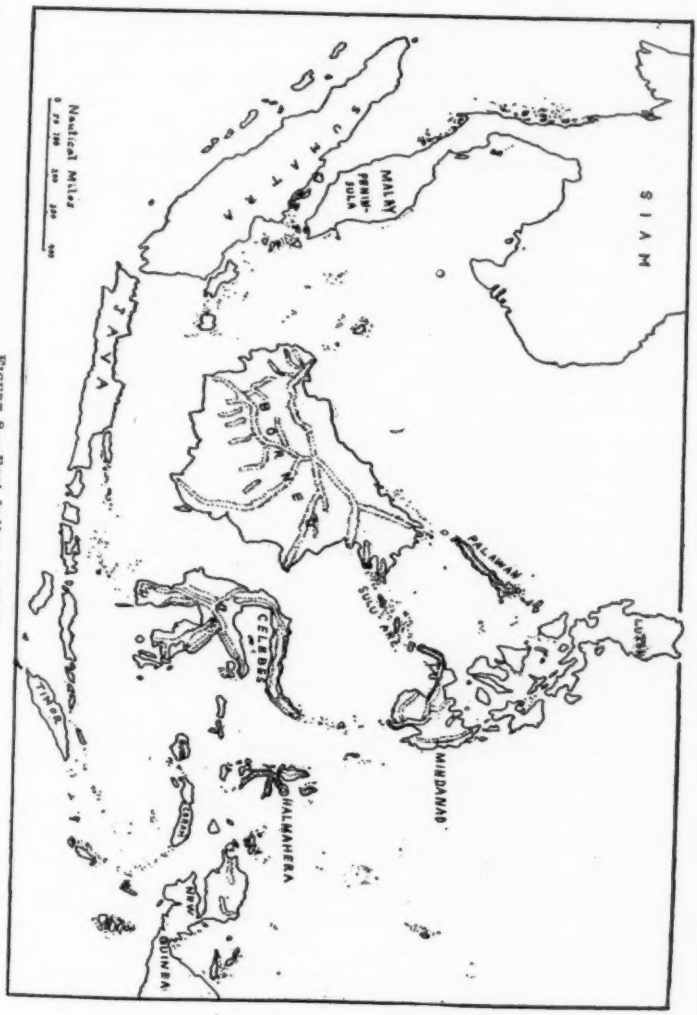


FIGURE 8.—*East Indian Archipelago*

Showing the peculiar mountain plans of Borneo, Celebes, and Halmahera, and their relations to the fold-lines of Asia and Australia.

by the Tertiary tangential movements. Its crustal movement was deflected to the west even more strongly than was that of North America—more strongly than any other continent, unless it be the New Zealand part of the Australian sheet (see figure 7). Only in the extreme northern part was the movement northward. A part of its Tertiary chains in Peru and Bolivia trending northwest and southeast were thrust from the northeast, and have therefore been deflected more than 90 degrees from the normal south-to-north direction, and the same is the case with New Zealand. The great arc of the northern Andes, extending from Venezuela to northern Chile, is convex toward the west—that is, toward the Pacific.

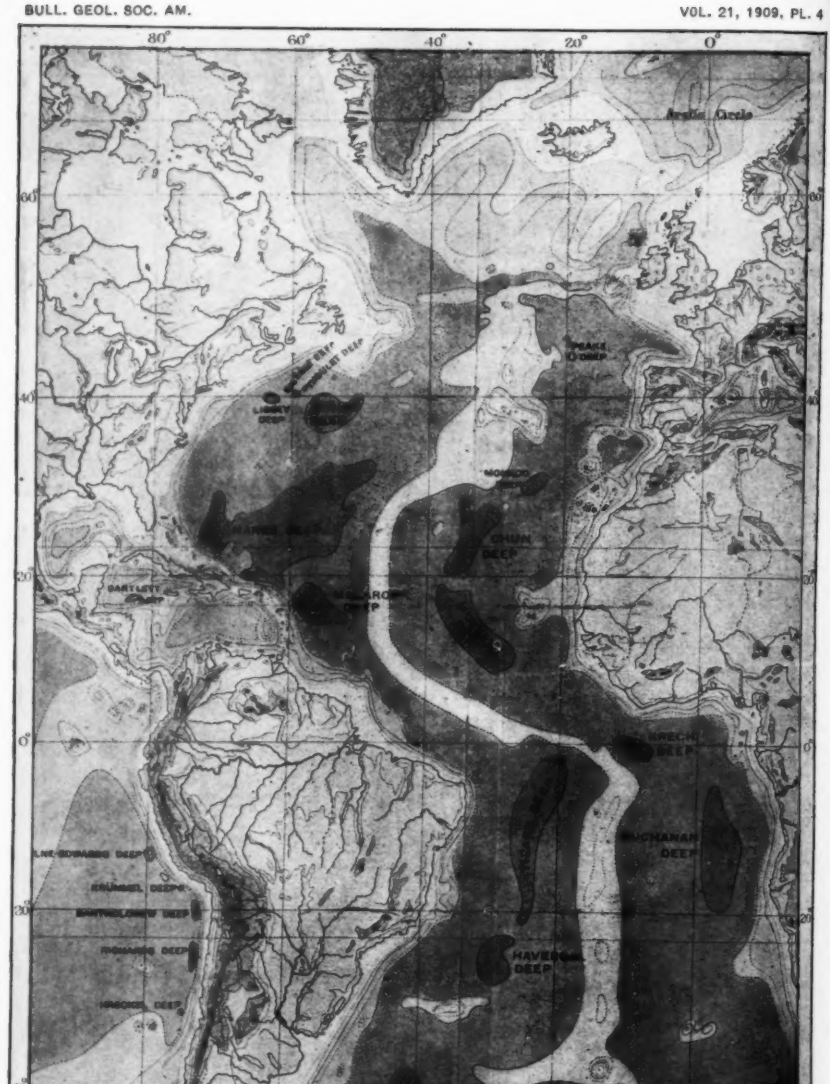
Here again the Cuzco knot of the Andes, one of the most remarkable mountain knots in the world, is somewhat interlobate in character—that is, it is in a re-entrant angle, where the crustal movements were slightly convergent, though less so than in Alaska. The Alps of New Zealand appear also to be at an angle of convergence or intersection.

The most peculiar character in South America, however, is the curvature of its Cordillera to the east at the southern extremity. It is as though the folds of the main Cordillera, being pushed toward the west, had lagged or dragged behind the rest at the extreme southern end. If this be true, it appears to indicate a minimum movement of 400 or 500 miles, which compares favorably with the movement of North America, as indicated by the rift of the Labrador Sea.

The curved ranges of the Antilles lie in the region of conflict between the two Americas, just as the irregular ranges of Europe lie between Europe and Africa and the peculiar mountain forms of Borneo, Celebes, and Halmahera lie between Asia and Australia. But the movements of the two Americas did not meet squarely, nor was one much more vigorous than the other. Both being relatively weak, there was an easier adjustment on curves of broader sweep. Nevertheless, the magnificent curve of the lesser Antilles reminds one of the sharper curves in Europe—the Roumanian, Alpine, and Betic arcs—and still more of the sharp curves of the Java and Timor lines, where they turn in such remarkable curves from east to north and back to the west to the island of Ceram (see figure 2). Even if the explanation of the plan of these ranges be regarded as still problematical, it is easy to see that they all occur in regions where there was a conflict of crustal movements and a tendency to a tangling and complication of folds.

THE MID-ATLANTIC RIDGE

One of the most remarkable and suggestive objects on the globe is the mid-Atlantic ridge. It is well shown on Sir John Murray's bathymetri-



BATHYMETRICAL CHART OF THE OCEANS

After Sir John Murray

cal chart of the oceans, referred to above, and that part of the chart showing the ridge is reproduced here in plate 4.

The persistence with which this feature maintains a medial position in the ocean bed for nearly 9,000 miles (following its great curves) is very striking, and the position which it takes in passing between South America and Africa is still more remarkable. The ridge is a submerged mountain range of a different type and origin from any other on the earth. It is apparently a sort of horst ridge—a residual ridge along a line of parting or rifting—the earth-crust having moved away from it on both sides. On the chart it is indicated to be mostly less than 2,000 fathoms (12,000 feet) beneath the sea, with some parts less than 9,000 feet and a few volcanic islands which rise 3,000 or 4,000 feet above the sea. Its general height above the surrounding ocean floor is between 3,000 and 6,000 feet, with a few island peaks rising 18,000 or 20,000 feet. Between South America and Africa the ridge runs east-southeast through 40 degrees of longitude (nearly 3,000 miles), and in this part is parallel with the adjacent continental border of South America.

The great westward bulge of Africa north of the equator appears to fit very closely into the westward bend of the mid-Atlantic ridge, suggesting that Africa has drifted eastward from that position. All authorities seem to agree, however, that Africa did not participate in the Tertiary folding, but remained stationary. Hence, if its western margin ever rested on the mid-Atlantic ridge it can hardly have been at a later time than the Carboniferous, for no important or extensive crustal movements appear to have affected Africa since that time. Too little is known of the geology of Africa, however, for settled conclusions now, but the narrow belt of peripheral folds on the southeastern border and also the high plateau of eastern Africa seem to be normal products of a crustal movement from the west-northwest.

At a first glance, the mid-Atlantic ridge appears to favor the torsion hypothesis of Prinz,[8] in which the southern hemisphere is supposed to have been rotated to the east relatively to the northern. The ridge certainly seems to suggest such a movement more definitely and precisely than any other feature yet described, but its form would seem to indicate that the yielding to the torsionary force was confined to a narrow zone near the equator, and that in that zone it had caused a displacement of nearly 3,000 miles. If the ridge ran originally straight north and south, and has been offset by torsion to give it its present form, then a section

[8] Dana's Manual of Geology, 4th ed., pp. 395-396. Also Bulletin of the Geological Society of America, vol. 11, 1899, pp. 93-94.

of it about 1,000 miles long has been stretched to a length of nearly 3,000 miles!

It is probably much nearer the truth to suppose that the mid-Atlantic ridge has remained unmoved, while the two continents on opposite sides of it have crept away in nearly parallel and opposite directions. The Cordillera of South America show that that continent moved a considerable distance toward the west and northwest in the Tertiary diastrophism; the movement of Africa appears to have occurred at a much earlier date, apparently before the Mesozoic era.

There are many bonds of union which show that Africa and South America were formerly united. Their present forms and relations suggest that the force which parted them was one that tended originally to crowd the two parts toward each other—that is, it tended to make Africa move south and South America north. But the release of strain was found by a great diagonal fracture along which the crust divided in two parts that crept away in opposite directions. The mid-Atlantic ridge remained unmoved and marks the original place of that great fracture.

THE ANTARCTIC LAND

This is the great southern horst, just as Greenland is the great northern, but its rôle in the Tertiary movements appears on present knowledge to have been less prominent. The pulling away from the Arctic regions was prodigious, especially toward Asia; only Greenland remained unmoved; whereas the whole of the Antarctic land appears to have held fast, while only Australia and South America pulled away. No doubt great rifts exist, corresponding to the pulling away of these two continents, but they appear to be submerged and obscured much more than those of the north. Perhaps the Jeffreys deep, south of Australia, stands in this relation to that continent, and possibly the Ross deep bears the same relation to South America.

THE SOUTHERN HEMISPHERE THE COMPLEMENT OF THE NORTHERN IN TERTIARY CRUSTAL MOVEMENTS

Although the facts are fewer and perhaps not so clear in their import, there seems still to be ample evidence that, excepting the southern part of the Malay arc, those parts of the Tertiary mountain belt which fall within the southern hemisphere were produced by crustal movements which were directed in general from south to north or in the opposite direction from those of the northern hemisphere.

The abstract statement made above may now be extended so as to include the whole earth, thus: In the Tertiary mountain making the crustal

sheets moved in general from high toward low latitudes in both hemispheres—that is, both polar areas were regions of crustal dispersion or spreading, and the continental sheets, excepting Africa, all crept toward the equatorial zone. Hence the general order or plan of deformation was the same for both hemispheres, and the two complementary halves taken together make a symmetrical whole.

Distribution of the deforming Force

If the foregoing conception of the manner of production of the Tertiary mountain ranges be erroneous it is, of course, useless to seek an explanation of the earth's plan by this means. It may be said, however, for this conception that it is built upon a foundation consisting solely of observed facts, without any dependence upon a preconceived idea of the cause of the crustal movements, and yet it reveals, in the distribution of the Tertiary mountain belt and in characters which show the relations of the several parts of that belt to the continental crustal sheets, a degree of systematic order and unity not reached by other methods, and this alone seems to justify further consideration.

It is worthy of note that no other great crustal movements than those described above appear to have occurred in the Tertiary diastrophism. Indo-Africa was not affected by tangential movement. If it was affected at all it moved only radially—that is, either up or down. There appear to be conflicting evidences on this point, but the great rift valleys of the lake region in Africa suggest moderate uplift. These valleys are roughly meridional and suggest a slight girth-expansion of the earth. This effect, however, is relatively small, and for the present purpose is negligible. The tangential movements affecting the other continents may therefore be taken as a substantially complete inventory of Tertiary deformation.

The facts of distribution seem to show plainly that the deforming forces were in some way conditioned by latitude, for vast crustal sheets moved toward lower latitudes from both poles, and these movements appear to have satisfied largely the stresses caused by the deforming forces. The effect appears to be the same as if the deforming forces had operated to flatten the earth at the poles. If Africa were slightly uplifted without tangential movement, this uplift and the meridional rifting may be an effect of equatorial bulging or girth-expansion. But whether Africa shows equatorial expansion or not, the remainder of the continents surely show polar flattening and crustal dispersion. The deforming force appears to have acted with maximum power in high latitudes, for the

mountain ranges produced by the Tertiary crustal movements are found mainly in middle and low latitudes—that is, along the lower margins of the crustal sheets.

Relation of the Tertiary crustal Movements to the Earth's Plan

Enough has been said in the preceding pages to show how the present plan of the earth has been affected by the Tertiary crustal movements. All the continents excepting Indo-Africa were affected and modified by them. As Suess observes, all the older parts of the continents were subjected to folding before the Tertiary, and yet it is certainly true that the Tertiary movements dominate largely in the earth's present plan and have given all the continents the larger part of their present outlines, excepting Indo-Africa.

One of the most remarkable things in the earth's plan is the fact that so much land appears to be clustered around the north pole and so little around the south pole. The northern hemisphere is largely continental, while the southern is mainly oceanic. In the northern hemisphere, however, Greenland is the only large remnant of the original north polar continent and most of the polar region is open sea, much of it deep, while in the southern the pole and the polar regions generally are occupied by the Antarctic continent, which is many times larger than Greenland. These peculiarities have not been explained, but if flattening of the poles with crustal dispersion characterized the Tertiary deformation as described above, then a simple explanation seems available.

Suppose the earth to have been originally a perfect sphere, and then to have been subjected to polar flattening, like that described above and suggested by Suess himself in one of his later works. This would change the earth to an oblate spheroid. The polar flattening, with a tendency to crustal dispersion, may be supposed to affect both poles at once and at first equally.

Now the first pole to undergo a crustal movement toward lower latitudes on a large scale would manifestly cause a slight shifting of the earth's center of gravity toward the other pole. Suppose the first large movement to have been from the north pole, then the earth's center of gravity would be shifted slightly toward the south pole. This would leave the remaining north polar lands under slightly greater strain than before, while the strain tending to dispersion of south polar lands would be proportionally diminished.

Such a change would, of course, increase the chances of further move-

ments from the north pole and decrease those from the south pole. Thus the pole from which the first great movement occurred would take the lead in crustal movements, and would continue henceforth to be the one from which the greatest movements would take place. The ocean would follow the shifting center of gravity, and thus would tend to draw away slightly from the north polar regions and rise slightly upon the south polar regions. Both of these effects tend to a concentration of land around the north pole and of water around the south pole. The stronger crustal creep from the north pole appears to have carried the land away from the immediate vicinity of that pole, while the feebler dispersion from the south pole has left a high, extensive land-mass in that region.

The southward tapering of the continents may also be related in some way to the dominance of crustal movements from the north pole. The strong dispersion from this pole and the drawing away of the water has left a girdle of land near the Arctic circle which is continuous, except for the narrow Bering Strait and the wider rifts on the east and west sides of Greenland. Even if the entire surface of the globe north of the 45th parallel of north latitude had been land at the beginning of the Tertiary movements, and if all the crust of this area had crept away southward into latitudes lower than the 45th parallel, it could not have filled the space south of this parallel with land, because of the great increase of area. At the same time, the north polar flattening appears to have been confined almost wholly to the area north of the 45th parallel. Hence, in moving southward there was a diminishing power of the deforming force and at the same time a very large increase of surface area. In these two circumstances there are elements which necessarily imposed limitations upon the southward extension of the continents, but they seem to suggest broad lobate forms like Asia and Australia, rather than tapering, pointed forms like North and South America. But the American and African forms may have arisen from the influence of preexistent meridional faults or lines of weakness which drew the crustal movements to one side.

Suess on the Cause of Deformation of Strand-lines

It does not fall within the scope of this paper to attempt any discussion of the ultimate causes of the Tertiary mountain making, nor of the more recent displacement of the strand-lines, but any suggestion by Suess upon these points is worthy of the most careful consideration. Because he has reviewed the field more extensively and more thoroughly than any other living man, we are naturally inclined to look to him more than to

any one else for an explanation of the causes of displacement of strand-lines and of mountain making. But Suess does not undertake to explain. He is content to make only very brief and apparently tentative suggestions concerning the nature of the causes.

It is interesting to observe that after relying upon eustatic negative movements through nearly all of part III of "The Face of the Earth" to account for displaced strand-lines, Suess turns in the closing pages of that part to a very different cause, where he says:

"Movements like these, which present themselves as oscillations, and extend around all coasts and under every latitude in complete independence of the structure of the continents, can not possibly be explained by elevation or subsidence of the land. Even as the transgressions of the ancient periods are much too extensive and uniform to have been produced by movements of the lithosphere, so, too, are the displacements of the strand-line in the immediate past" (II, 550).

After rejecting Adhémar's suggestion of an alternating accumulation of water at each of the poles, he observes:

"As far as we are in a position to judge, it appears much more as if that which characterized the more recent movement was an accumulation of water toward the equator, a diminution toward the poles, and as though this last movement were only one of the many oscillations which succeed each other with the same tendency—that is, with a positive excess at the equator, a negative excess at the poles.

"Negative traces are to be seen in all latitudes. We may attempt to explain them by means of eustatic negative processes—that is, by great subsidences; but this would presuppose a uniform sinking of the sealevel to the extent of more than 1,000 feet in quite recent times. It is much more probable that the negative traces at considerable altitudes in the tropics are not of the same age as those in high latitudes, and that an accumulation of water occurs alternately at the poles and the equator. Among these traces there may be one or more of a eustatic negative origin, but if so, we have not yet learned how to distinguish them" (II, 551).

Thus, although he had relied in earlier chapters almost exclusively upon subsidence as the one great agency of change in the earth's history, Suess seems constrained to admit in the last analysis that most of the displaced strand-lines appear to have been caused by an independent oscillatory movement of the ocean. Suess calls this movement an oscillation; but what kind of an oscillation is it? He answers this question clearly enough, where he says that "an accumulation of water occurs alternately at the poles and the equator."

The present figure of the ocean is that of an oblate spheroid—a spheroid having a certain degree of oblateness which is roughly expressed by the fact that the polar diameter of the globe is 26 miles

shorter than the equatorial diameter. If the present phase of oscillation is one in which, as Suess says, there is an accumulation or positive excess of water at the equator and a negative excess at the poles, then the climax of the next preceding phase was one in which the positive excess was at the poles and the negative at the equator, and the degree of oblateness was less than it is now. Suess does not give a name to the oscillation which he describes, but he might as well have done so, for obviously it is merely an oscillation of the oblate figure—that is, a change from one degree of oblateness to another and back again, repeated time after time. The latest change, according to Suess, was an increase of oblateness.

The case could not be more clearly or forcefully stated than it is by Suess, so far as relates to the ocean. But he stops there. Yet, how is it possible to confine a force which changed the figure of the sea to the sea alone? The same force must have been exerted at the same time and with the same power upon the land—that is, on the lithosphere or solid globe. And if it were, what would be the nature of the stresses set up and what movements might be expected when those stresses were relieved?

Manifestly, on Suess' idea the present surface of the ocean is lower in the polar regions than it was some time in the relatively recent past and higher at the equator. The new figure intersects the old on a parallel a little below the 45th in both hemispheres. The lands in the far north therefore stand relatively higher above the sea, or rather above the ideal mean surface plane of the solid globe,[9] than before and at a slightly different angle to this plane at all points, excepting at the poles and the equator. In middle latitudes there is a slight change of angle, but no appreciable change of altitude.

Any imaginary plane or surface which before the change lay parallel with the surface of the sea in northern latitudes would now have the appearance of slanting slightly downward toward the south. It would seem certain that such a change as this would be fraught with tremendous consequences to the solid globe, for the relative increase in altitude of all lands in high latitudes would disturb the preexisting equilibrium and increase the stresses in those lands tending to subsidence. But the earth being solid from its surface to its center, and more rigid than the hardest steel, the tendency to sink directly toward the center could not be realized. The more rigid central body would resist effectively, and the forces would consequently be deflected from inward radial to tangential forces radiating from the pole and affecting only a relatively

[9] Estimated by Gilbert, on data collected by Sir John Murray, to be about 9,000 feet below sealevel. In his International Geography, H. R. Mill puts it at about 7,500 feet.

thin crust. These forces would tend to cause the crust to creep away on lines of dispersion from the pole.

An imaginary plane, like that mentioned above, gently sloping toward lower latitudes and situated beneath the earth's crust just within the zone of rock flowage, would seem to afford a basal slope down which the crustal sheets might move, and the tangential thrust forces exerted in the crust toward lower latitudes would tend to give the crust the requisite impulse to move.

We may thus enumerate, in part, the conditions and the tendencies to earth-movements which might be expected to affect the lithosphere on the supposition of a flattening of the poles, like that postulated by Suess in explanation of displaced strand-lines.

Conclusion

The displacement of strand-lines by eustatic negative movements is a very different thing from displacement by oceanic oscillations. As was pointed out above, it is only at the end of his exhaustive review of the whole subject that Suess recognizes and accepts the principle of oceanic oscillation. After accepting this principle in explanation of the strand-lines, it seems a little strange that Suess should have made no mention in "The Face of the Earth" of the possibility that the same force which caused oscillation of the ocean might be a cause of crustal movements also; but there appears to be no suggestion of this idea, at least not in the first four parts.[10]

In a later work, however, Suess seems to see the possibility of deformation of the lithosphere by a force that flattens the poles in the same manner as he supposes oscillations of the ocean to do for the strand-lines. He says: "One is inclined to suspect that the formation of the curved chains in Asia, open to the north, stands in some connection or other with the outflow of superfluous earth-mass from the pole—that is, with a flattening of the same."[11]

Perhaps these words are not sufficiently explicit to enable one to say that Suess accepts the idea that the forces associated with an increased oblateness of the earth's figure are the cause of polar flattening of the lithosphere, but it is hard to see how he could have had any other cause

[10] Volume IV, containing Part V of Suess' "Face of the Earth," reached the writer a few days before the proof of this paper. A hasty examination disclosed nothing that suggests any important change in the conclusions reached. Chapters VII, IX, X, XI, XIV, XVI, and XVII are of particular interest in connection with this paper.

[11] E. Suess: Asymmetry of the Northern Hemisphere. Appendix to the presidential address of B. K. Emerson. Bulletin of the Geological Society of America, vol. 11, 1899, p. 105. Published in German in 1898.

in mind. Whatever the cause may have been, its distributive characters appear to be precisely the same as those which belong to an increase of oblateness of the oceanic figure. Why should we look for two separate causes for two sets of phenomena which appear to require precisely the same kind and distribution of force to produce them?

The first author to suggest oscillations of the figure of the ocean as the cause of displaced strand-lines appears to have been Emmanuel Swedenborg. Suess traces this idea back to him through Robert Chambers and P. Frisi (II, 7, 11, 16, and 21). But whether Suess regards increased oblateness as the cause of crustal movements or not, it seems certain that he would have reached this conclusion long ago if he had held steadfastly to his first conception that the mountain ranges of western North America were folded toward the Pacific, instead of receding from that position in deference to the contrary opinions of certain American and Canadian geologists; for if he had found North America folded toward the Pacific Ocean, he would also have found that the evidence of general crustal creep and dispersion from high toward low latitudes is complete for the northern hemisphere.

All forms of the contraction hypothesis meet with two insurmountable difficulties with reference to the Tertiary period of mountain making. They fail to explain in a satisfactory way the distribution of Tertiary mountain ranges upon the earth's surface, and they do not explain how so great a period of mountain making could have occurred in so recent time. If due to contraction arising from cooling, it is necessary to suppose a very long period of accumulation and storage of mountain-making force before the beginning of the folding movement. The amount of crustal movement which occurred during the Tertiary period seems to be far in excess of the most that can be attributed to cooling and shrinking since the time of the Permo-Mesozoic (Appalachian) folding, even on the most liberal estimate. It is scarcely credible that any considerable mountain-making force derived from cooling before the time of the Permo-Mesozoic folding could have survived that event so as to be an important element in the Tertiary folding.

Referring to the tangential crustal movements in Asia, Willis, in a recent development of the contraction hypothesis based on isostacy, says: "What Suess considers an outward advance, I regard as a retarded superficial layer, beneath which the deeper mass has been squeezed northward into narrower space."[12] Willis supposes subsidence of the earth-segment under the Pacific and Indian Oceans, with northward spreading or un-

[12] Bailey Willis: Research in China, vol. 2, Systematic geology. Carnegie Institution of Washington, 1907, p. 126.

derthrusting of the deep-seated mass beneath Asia. By this hypothesis the crustal sheet in which the folding, overthrusting, etcetera, are produced is not the part in which the principal movement occurs. The crustal sheet is regarded as a passive element, and the mountains of Asia exist only because some other part of the earth than the crust moved horizontally and the crust became involved in that movement. But if the same results in mountain making can be derived from horizontal movements of the crust alone, why postulate a more complicated indirect process?

Even Chamberlin's Planetesimal hypothesis, which has brought so great an advance over the Nebular hypothesis of Laplace with reference to the origin and growth of the earth, meets these same two difficulties, and with no better success.[13]

It seems certain that no man living has ranged so widely over the fields of geology for the entire earth as Eduard Suess. He appears to have made himself familiar with every official report and every important memoir or scientific contribution that has ever been written on the subject. His earlier studies turned his mind along certain lines of interpretation, chiefly depression of oceanic basins and tangential crustal movements overthrusting those basins. This was natural, for every one must arrange his thoughts around some central idea as he goes on working year after year, decade after decade. Indeed, one's thoughts will inevitably crystallize themselves around some uniting principle, whether he will or no. These early principles of interpretation served Suess well throughout the greater part of his life. But is it not deeply significant that after a lifetime of study along the lines of those early principles, Suess at last leans toward a different interpretation, both as to the cause of displaced strand-lines and of deformations of the lithosphere? And is it not still more significant that in both instances his leaning is toward a cause which conforms precisely with increased oblateness of the earth's figure, or with oscillations of the same?

The argument presented in this paper rests at last on the truth of Suess' interpretation of the mountain plan of Asia. The principles which he worked out there have been applied without important modification to the other continents, and the conclusions reached in this way appear to accord very closely with suggestions made by Suess himself in his later writings. For a change in the degree of oblateness, either in oceanic oscillations or in deformations of the lithosphere, one is inclined to reject all internal causes and to look to some form of tidal force as the only possible agency.

[13] Chamberlin and Salisbury: Geology, vol. II, pp. 82-132.

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ISOBASES OF THE ALGONQUIN AND IROQUOIS BEACHES, AND THEIR SIGNIFICANCE[1]

BY JAMES WALTER GOLDTHWAIT

(*Read before the Society December 28, 1909*)

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INTRODUCTION

The study of epeirogenic movements by means of critical surveys of raised beaches is one for which North America is a peculiarly rich field. The investigations of Gilbert in Lake Bonneville, of Upham in Lake Agassiz, and of Gilbert, Spencer, Leverett, Taylor, Fairchild, Coleman, Woodworth, and others in the Great Lakes region have contributed greatly to the understanding of post-glacial upwarpings. Among the many papers which have been written on this subject, one is unique in that it seeks to correlate observations over all eastern North America. This is the paper read before the Boston Society of Natural History in 1892 by Baron Gerard De Geer.[2]

[1] Manuscript received by the Secretary of the Society January 12, 1910.

[2] Gerard De Geer: On Pleistocene changes of level in eastern North America. Proceedings of the Boston Society of Natural History, vol. 25, 1892, pp. 454-477.

This distinguished visitor, fresh from studies of the upwarped marine strands of Scandinavia, applied himself at once to the task of correlating the measurements of raised beaches which had been made in New England, the maritime provinces, the Great Lakes region and the Northwest; and this correlation he reinforced with hurried though accurate observations of his own in New England and southeastern Canada. In order to show the upwarped form of the old "geoid surface" which he reconstructed, De Geer used a device which Gilbert had used ten years before in connection with Lake Bonneville[3]—he drew curves of equal deformation of the ancient water-plane. These he named "isanabases," or "isobases."[4] They were not limited to the Atlantic Coast district which De Geer had personally examined, but were extended far into the interior, on the basis of measurements already secured around the Great Lakes by Gilbert, Spencer, Upham, and Todd, and in Labrador and the Hudson Bay region by Bell and Low. In view of the scarcity of data which De Geer had at his command and of the difficulties in correlation which he met by "interpolating" his geoid surface beneath deformed lake beaches of unknown ages, it is no wonder that his isobases for the Great Lake region hit wide of the mark. It would hardly have been surprising if his conclusions had been thoroughly discredited by later detailed investigations. On the contrary, however, De Geer's most fundamental conclusions seem to have been confirmed. The analogy which he drew between the North American and the Scandinavian uplifts appears to hold good; in both cases the movements were differential uplifts of a glaciated area, and in both the isobases follow so closely the boundary of pre-Cambrian areas that a causal connection between pre-Cambrian oldlands, glaciation, and differential uplift is strongly suggested.

Since De Geer's map was published, the use of isobases seems to have been limited almost wholly to private studies; very few isobasic maps have been published, and even these concern districts of comparatively small extent. This has probably been due in most cases to the fact that measurements have not been numerous or accurate enough to make the isobases convincing, or even passably satisfactory. The fault has been in the data rather than in the method. One can easily see that an isobasic map is as valuable to the investigator of epeirogenic movements as a contour map is to the physiographer. If the data which we possess are too

[3] G. K. Gilbert: Contributions to the history of Lake Bonneville. Second Annual Report of the U. S. Geological Survey, 1882, pp. 195-197, plates xlii and xliii.

[4] Gerard De Geer: Quaternary changes of level in Scandinavia. Bulletin of the Geological Society of America, vol. 3, 1892, p. 66; and op. cit., Proceedings of the Boston Society of Natural History, vol. 25, 1892, p. 457.

incomplete to justify the drawing of isobases, our efforts ought to be redoubled to accumulate data so accurate and so abundant that a set of isobases shall provoke no more skepticism than so many well-executed contour lines. Then, and not until then, can we hope to do much in analyzing the great upwarpings of late glacial time.

Good isobasic maps are valuable in two ways: First, as aids to fixing the relative ages of extinct lakes in different parts of a region; and, second, as indicators of the extent, nature, and cause of epeirogenic movements. The present paper touches both sides of the subject. Isobases for the Iroquois beach and the Algonquin beach will be compared, with a view to answering the question whether these two critical stages of the extinct lakes, Iroquois and Algonquin, are synchronous or not. The question of the nature and cause of the differential uplifts will be treated very briefly, because that is soon to be taken up by Mr. F. B. Taylor in a monograph which is now in preparation.

The data here used are from several sources, as references in every case will show. Most of the measurements of altitude of the Algonquin beach have been made during the last five years by the geological surveys of Wisconsin and Michigan, the U. S. Geological Survey, and the Geological Survey of Canada. These measurements are especially reliable, because they were made with the wye-level, rather than the hand-level or the aneroid barometer.

The Algonquin Water-plane

Stage recorded by the Algonquin beach

The Algonquin beach marks a critical stage in the history of Lake Algonquin. For some time previous the discharge of the ice-dammed lake had been entirely through the Trent Valley into the waters of the Lake Ontario basin along the "Algonquin River"[5] (see figure 1). Differential uplifts, however, had been lifting this region with respect to more southerly districts, and the rising waters of the lake had been advancing on the shores in the Michigan and Huron basins. When the head of the Trent Valley, at Kirkfield, Ontario, had been lifted to an altitude as high as the pass at the south end of Lake Huron, that pass began to receive a share of the overflow which ran down the Saint Clair and Detroit rivers into Lake Erie. This two-outlet stage of the lake is

[5] The names "Lake Algonquin," "Algonquin beach," and "Algonquin River" were first used by J. W. Spencer: Notes on the origin and history of the Great Lakes of North America. Proceedings of the American Association for the Advancement of Science, vol. 37, 1889, p. 199.

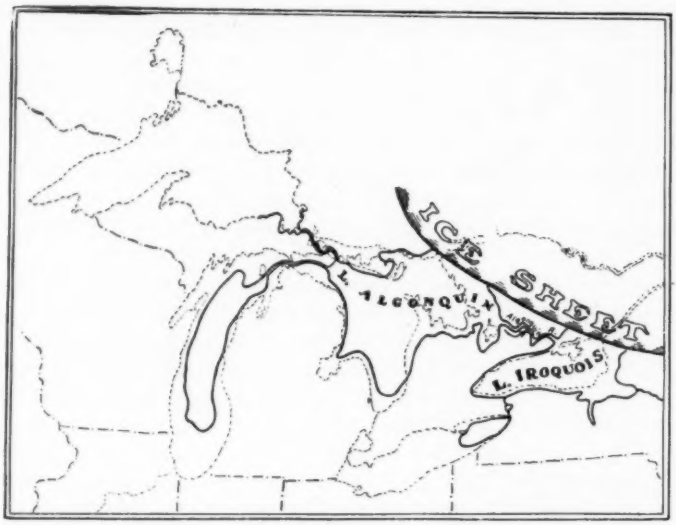


FIGURE 1.—*Map of Lakes Algonquin and Iroquois*

A short time previous to the date of construction of the Algonquin beach. (After Gilbert)

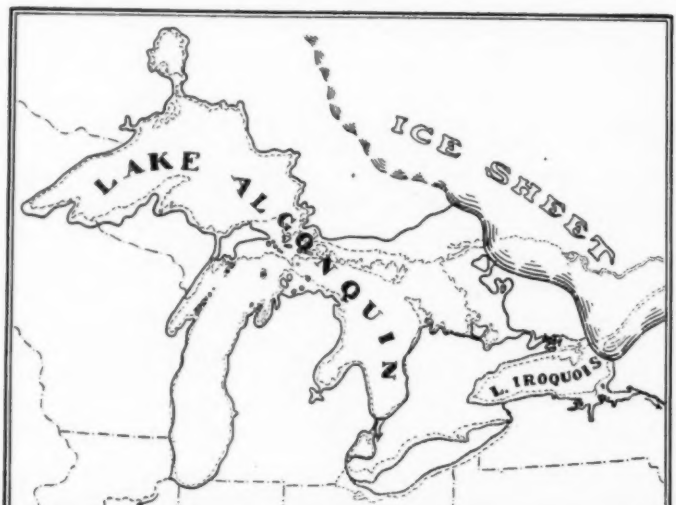


FIGURE 2.—*Map of Lake Algonquin*

At the stage marked by the Algonquin beach. The existence of Lake Iroquois at this time is doubtful. The position of the ice border is quite hypothetical

the stage marked by the Algonquin beach (see figure 2). It lasted probably but a short time, for as the canting of the region proceeded the discharge was shifted entirely into the Saint Clair River, while the Trent Valley and surrounding shores of the lake emerged from the water. Thus a beach which had been under construction perhaps for centuries, during the regime of the Algonquin River and the slow rising of Lake Algonquin, was suddenly abandoned as the water-plane adjusted itself to the new outlet. The canting of the abandoned water-plane progressed until the beach at Kirkfield had been lifted 275 feet higher than it had stood during the two-outlet stage.[6]

Today the Algonquin beach can be followed somewhat continuously from the old outlet at the head of the Saint Clair River, where it stands 607 feet above sealevel,[7] to the head of the more northerly outlet at Kirkfield, where its altitude is 883 feet. From here down the Trent Valley the course of the ancient Algonquin River is marked by a chain of extinct lakes and rapids which terminated in the contemporary waters of the Lake Ontario basin.[8]

ISOBASES OF THE UPWARPED PORTION OF THE ALGONQUIN PLANE

On the isobase map (plate 5) about 100 localities where the altitude of the Algonquin beach has been accurately determined are shown by round dots. In about 60 of these cases the measurements themselves are indicated in feet above sealevel. They are as follows:

Locality	Altitude, feet	Authority
West side of Lake Michigan:		
Burnt Bluff	723	W. H. Hobbs, Michigan Geological Survey, 1907.
Deaths Door	660	J. W. Goldthwait, Wisconsin Geological and Natural History Survey, 1905.
Rowleys Bay	654	Ditto.
Oconto	620	Ditto.
Sturgeon Bay	621	Ditto.
Cormier	611	Ditto.
Algoma	610	Ditto.
Two Rivers	607	Ditto.
Evanston	605	J. W. Goldthwait, 1906.
Rogers Park	604	Ditto.

[6] This sequence of events was first recognized by J. W. Spencer, loc. cit. It has been confirmed by observations of Gilbert and Taylor.

[7] Since Saint Clair River became the sole line of discharge of the lake, it has been deepened enough to lower the level of the waters in the Huron basin from 607 feet to 581 feet above sealevel.

[8] Gilbert: The Algonquin River (Abstract). American Geologist, vol. 18, 1896, p. 231.

Straits of Mackinac and east side of Lake Michigan:

Hessel	863	F. B. Taylor and J. W. Goldthwait, U. S. Geological Survey, 1907.
Mackinac Island	812	Ditto.
Cross Village	746	Ditto.
Beaver Island	731	Ditto.
Harbor Springs	709	Ditto.
Norwood	674	Ditto.
Northport	658	Ditto.
N. Manitou Island	648	Ditto.
Traverse City	619	Ditto.
Frankfort	605	Ditto.
Herring Lake	607	Ditto.
Arcadia	603	Ditto.
Muskegon	604 (?)	Ditto.
Spring Lake	603	Ditto.
Holland	604	Ditto.

West side of Lake Huron:

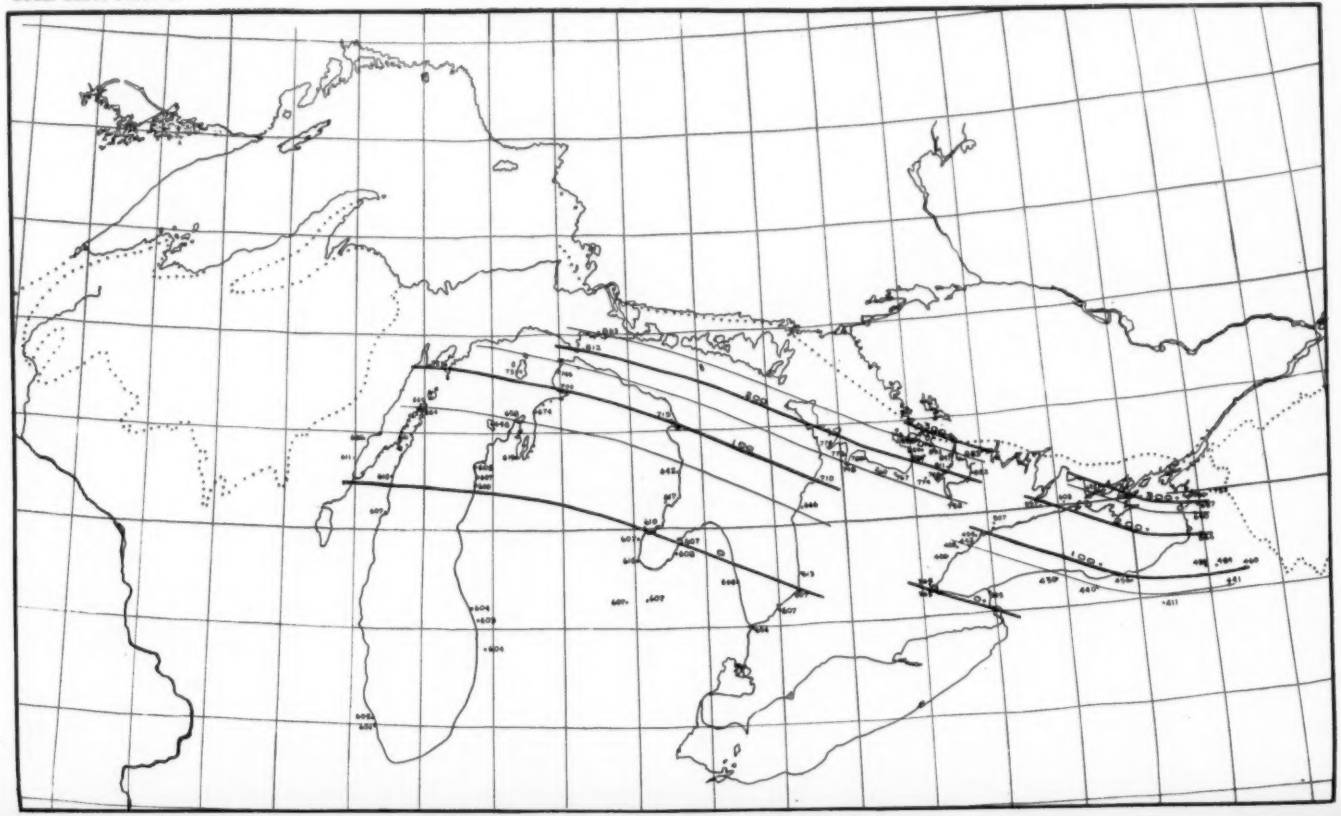
Alpena	713	Frank Leverett, U. S. Geological Survey.
Greenbush	642	Frank Leverett, Michigan Geological Survey, 1901.
Tawas	617	W. M. Gregory, Michigan Geological Survey, 1901 and 1907.
Omer	610	Ditto.
Worth	607	Ditto.
Kawkawlin	610	W. F. Cooper, Michigan Geological Survey, 1905.
Bayport	607	A. C. Lane, Michigan Geological Survey, 1900.
Sebewaing	605	Ditto.
Bridgeport	607	W. F. Cooper, Michigan Geological Survey, 1908.
Saint Charles	607	Ditto.
Port Sanilac	605	Ditto.

East side of Lake Huron:

Port Elgin	710	F. B. Taylor and J. W. Goldthwait, Canadian Geological Survey, 1908.
Kincardine	666	Ditto.
Bayfield	613	Ditto.
Grand Bend	607	Ditto.
Kettle Point	607	Ditto.
Sarnia	604	Ditto.

Georgian Bay and Penetang Peninsula:

Wiarton	776	Ditto.
Hogg	778	Ditto.
Owen Sound	748	Ditto.



ISOBASIC MAP OF THE ALGONQUIN AND IROQUOIS BEACHES
Showing the greater features of warping

Georgian Bay and Penetang Peninsula—Continued:		
Meaford	783	F. B. Taylor and J. W. Goldthwait, Canadian Geological Survey, 1908.
Clarksburg	769	Ditto.
Mair Mills	767	Ditto.
Colwell	774	Ditto.
Penetang	855	Ditto.
Wyebridge	840	Ditto.
Elmvale	829	Ditto.
Coldwater	852	Ditto.
Lake Simcoe district:		
Orillia	847	Ditto.
Oro	811	Ditto.
Barrie	785	Ditto.
Holland Landing	752	Ditto.
Beaverton	822	Ditto.
Kirkfield	833	Ditto.

With hardly an exception these measurements were made with the wye-level.

In regard to the data from localities east of Lake Huron and Georgian Bay, it should be said that the altitude of the Algonquin beach at most of these places was measured by Dr. J. W. Spencer in 1887. With only two or three exceptions the new measurements of 1908 agree with those of Spencer, and bear witness to the accuracy of his work.

Isobases for every 50 feet of uplift between 0 and 300 have been drawn with reference to these localities. That the isobases accord properly with the 60 stations whose measurements have just been tabulated is easily seen on plate 5. The accordance would be found to be just as good, however, if one selected at random any 60 of the 100 stations whose positions are shown by the dots. Detailed work on large-scale maps, the results of which have been published in other papers,[9] testify to the remarkably complete harmony between isobases and measurements.

In preparing the following summary of the attitude of this warped water-plane, such details as directions and rates of tilt have been worked

[9] J. W. Goldthwait: Correlation of the raised beaches on the west side of Lake Michigan. Journal of Geology, vol. 14, 1906, pp. 411-424.

Abandoned shorelines of eastern Wisconsin. Bulletin of the Wisconsin Geological and Natural History Survey, no. 17, 1907.

A reconstruction of water-planes of the extinct glacial lakes in the Lake Michigan basin. Journal of Geology, vol. 16, 1908, pp. 459-476.

Preliminary report on measurements of altitude of the Algonquin and Nipissing shorelines in Ontario, July 6-August 11, 1908. Geological Survey of Canada, Summary Report of Director for 1908, pp. 112-114.

An instrumental survey of the shorelines of the extinct lakes Algonquin and Nipissing in southwestern Ontario. Geological Survey of Canada (in press).

ISOBASIC MAP OF THE ALGONQUIN AND IROQUOIS BEACHES

out from maps whose scales range from 1:400,000 to 1:250,000. Plate 5, prepared from a much smaller map, is intended merely to show the broader features of warping, not to serve as a basis for estimating minute details.

Over the northern, or, more accurately, the northeastern, part of the Michigan-Huron basins the Algonquin beach has been upwarped so that it now slants southward at a rate that decreases down the slope of the plane. The inclination ranges from about 5 feet per mile in the north to 3 feet, 2 feet, 1 foot and less toward the south, to where the beach actually becomes horizontal, over the southern half of Lake Michigan.

The directions and rates of tilt in different districts, worked out after all available wye-level measurements had been plotted on large-scale maps, are tabulated in detail on page 235.

These figures have been tabulated again, in a more compact form, for the sake of comparison of tilt rates at corresponding altitudes in the different districts.

Tilt Rates of the Algonquin Beach arranged for Purposes of Comparison.

District.	Tilt rates, in feet per mile, between successive isobases.					
	0–50	50–100	100–150	150–200	200–250	250–300
Lake Michigan and Straits of Mackinac	1.16	1.82	2.85	3.33	3.40	.....
West side of Lake Huron	0.75	2.22	.....	.....	.....	.....
East side of Lake Huron and south side of Georgian Bay	1.02	2.10	2.11	.....	.....	.....
Lake Simcoe district	.....	.....	.....	2.68	3.00	6.00
Average tilt rates	0.97	2.05	2.48	3.09	3.20	6.00

Not a small part of the discrepancies between tilt rates in the same vertical column in this table may be due to the necessity of taking stations which do not lie exactly on the isobases, and whose differences of altitude are consequently not 50 feet, but range from 32 feet to 71 feet. Detailed isobase maps drawn on a large scale show that the tilt rate is more uniform between isobases than one might at first suppose from the

Table showing the Tilt Rates of the Algonquin Beach.

District.	Between what isobases (approximately.)	From—	To—	Direction of tilt.	Descent (feet).	Distance (miles).	Tilt rate (feet per mile).
Over the Straits of Mackinac and the west side of Lake Michigan.	250 to 200	Hessel, 863 feet.	Mackinac I., 812 feet.	S. 15° W.	51	15	3.40
	200 to 150	Mackinac I., 812 feet.	Cross Village, 746 feet.	S. 15° W.	66	20	3.33
	150 to 100	Cross Village, 746 feet.	Harbor Springs, 709 feet.	S. 10° W.	37	17	2 85
	100 to 50	Harbor Springs, 709 feet.	Northport, 658 feet.	S. 9° W.	51	28	1.82
	50 to 0	Northport, 658 feet.	Herring Lake, 607 feet.	S. 5° W.	51	44	1.16
Over the west side of Lake Huron.	100 to 50	Alpena, 713 feet.	Greenbush, 642 feet.	S. 21° W.	71	32	2.22
	50 to 0	Greenbush, 642 feet.	Omer, 610 feet.	S. 21° W.	32	44	0.73
Over the south side of Georgian Bay and the east side of Lake Huron.	150 to 100	Owen Sound, 748 feet.	Port Elgin, 710 feet.	S. 21° W.	38	18	2.11
	100 to 50	Port Elgin, 710 feet.	Kincardine, 666 feet.	S. 21° W.	44	21	2.10
	50 to 0	Kincardine, 666 feet.	Grand Bend, 607 feet.	S. 21° W.	59	58	1.02
Over Lake Simcoe.	300 to 250	Kirkfield, 883 feet.	Orillia, 847 feet.	S. 21° W.	36	6	6.00
	250 to 200	Orillia, 847 feet.	Oro, 811 feet.	S. 21° W.	36	12	3.00
	200 to 150	Oro, 811 feet.	Holland Landing, 752 ft.	S. 21° W.	59	22	2.68

table. Plate 5, although a small-scale map, does not exaggerate the uniformity.

THE HORIZONTAL PORTION OF THE ALGONQUIN PLANE

Around the southern half of Lake Michigan, and near the south ends of Saginaw Bay and Lake Huron, the Algonquin beach is almost, if not quite, horizontal. The identity of the beach is somewhat obscured by the presence of another strong shoreline, the Nipissing, which stands only about 12 feet below the Algonquin in both basins and which replaces it for long distances where cliff recession has been rapid and long continued.[10] Moreover, cliff recession along the modern lake front has destroyed considerable stretches of both of these ancient strands. Nevertheless, the data collected seem complete enough to show that the Algonquin beach is virtually horizontal over this wide area.

The following table gives the altitudes of the Algonquin beach or its supposed equivalent at 19 localities in or near the region of horizontality. All the measurements except the one at Port Sanilac were made with the wye-level:

Altitudes of the Algonquin Beach in the Region of Horizontality.

Locality.	Altitude.	Authority.	Reference.
	Feet.		
Two Rivers, Wisconsin..	607	Goldthwait.	Bulletin Wisconsin Geological and Natural History Survey, No. 17, 1906, page 60, figure 11, and plate 12.
Evanston, Illinois.....	605	Goldthwait.	Bulletin Illinois Geological Survey, No. 7, 1908, pages 64–65.
Rogers Park, Illinois	604	Goldthwait.	Idem, page 66.
Holland, Michigan.....	604	Taylor and Goldthwait.	U. S. Geological Survey.
Spring Lake, Michigan..	603	Taylor and Goldthwait.	U. S. Geological Survey.
Muskegon, Michigan	604	Taylor and Goldthwait	U. S. Geological Survey.
Arcadia, Michigan.....	603	Taylor and Goldthwait.	U. S. Geological Survey.
Herring Lake, Michigan.	607	Taylor and Goldthwait.	U. S. Geological Survey.
Frankfort, Michigan.....	605	Taylor and Goldthwait.	U. S. Geological Survey.
Worth, Michigan.....	607	Taylor and Goldthwait.	U. S. Geological Survey.

[10] See Abandoned shorelines of eastern Wisconsin. Bulletin of the Wisconsin Geological and Natural History Survey, no. 17, 1906, pp. 44-45; and Reconstruction of water-planes of the extinct glacial lakes in the Lake Michigan basin. Journal of Geology, vol. 16, 1908, pp. 459-476. In the latter the altitude of the Algonquin beach was placed provisionally at 596 feet instead of 607 feet. The statement was made, however, that this 596-foot beach might prove to be the Nipissing, and that the Algonquin might be expected at 607 feet (see p. 472).

Altitudes of the Algonquin Beach in the Region of Horizontality—Continued.

Locality.	Altitude.	Authority.	Reference.
	Feet.		
Bayport, Michigan.....	607	Lane.	Michigan Geological Survey, Annual Report for 1900, volume 7, part 2, 1900, pages 47–51, and 75–76.
Sebewaing, Michigan...	{605 / 611}	Lane.	Idem, pages 47 and 49.
Kawkawlin, Michigan...	610	Cooper.	Michigan Geological Survey, Annual Report for 1905, 1906, page 349.
Bridgeport, Michigan....	607	Cooper.	Tenth Annual Report, Michigan Academy of Science, 1908, pages 97–98.
St. Charles, Michigan....	607	Cooper.	Loc. cit.
Port Sanilac, Michigan...	605	Leverett and Taylor.	Information given by Mr. Leverett.
Sarnia, Ontario.....	605	Taylor and Goldthwait.	Canadian Geological Survey, Summary Report for 1908, 1909, pages 112–114.
Kettle Point, Ontario....	607	Taylor and Goldthwait.	Canadian Geological Survey, Summary Report for 1908, 1909, pages 112–114.
Grand Bend, Ontario....	607	Taylor and Goldthwait.	Canadian Geological Survey, Summary Report for 1908, 1909, pages 112–114.

Between the first two localities in this table there is a stretch of 150 miles in which no remnant of a beach at the 605-607-foot level has been discovered. The beach at Two Rivers is surely the Algonquin: the one at Evanston until recently was regarded as the shoreline of a local lake of the Michigan basin—Lake Chicago. The reasons for correlating it with the Algonquin are: (*a*) its strength; (*b*) its uniform altitude of 603-610 feet around the south end of Lake Michigan; (*c*) the horizontal attitude of the Glenwood and Calumet beaches of Lake Chicago, which stand above it; (*d*) the presence in it of molluscan shells, which are absent from the higher beaches; (*e*) the occurrence of forest beds beneath the beach gravels, believed to record the stage of low water during the earlier part of the activity of Algonquin River, and (*f*) evidences of erosion of cetrain valleys tributary to Lake Michigan to low levels, previous to the construction of this beach, as if to record the low-water stage just mentioned.[11]

[11] On these points see Leverett: The Illinois glacial lobe. Monograph of the U. S. Geological Survey, vol. 38, 1899, pp. 445-453.
W. C. Alden: Chicago Folio, No. 81, Geological Atlas of the United States, U. S. Geological Survey, 1902, pp. 10-11.
Goldthwait: Bulletin of the Wisconsin Geological and Natural History Survey, no. 17, 1906, pp. 109-111, 118; and Bulletin of the Illinois Geological Survey, no. 7, 1908, pp. 65-66.

For the Lake Michigan district these measurements give an average of 605 feet. Measurements with hand-level and aneroid barometer and contours of a large-scale map of the Sanitary District of Chicago agree in putting the height of the "Toleston," or "Algonquin," beach close to 600-605 feet around the south end of Lake Michigan. At the south ends of Saginaw Bay and Lake Huron the measurements average 607 feet. The altitudes at all 19 localities range from 603 to 610 feet. All of this variation of 7 feet can be accounted for by original differences in height of construction of the beach by the lake.

Even if we take the strong 596-foot (Nipissing) shoreline of the Lake Michigan basin as the equivalent of the 607-foot beach of the Lake Huron basin (although in the latter basin also is a strong shoreline at 596 feet, which seems surely to be the equivalent of the Nipissing beach of the Michigan basin), the extreme variation in height is between 593 feet and 610 feet, and of 40 wye-level measurements already recorded only one is below 595 feet. Thus for 350 miles from east to west and for 200 miles from north to south there is a variation of scarcely 17 feet in altitude, at most, of the Algonquin beach, and, if the distinction between the Algonquin and the Nipissing shorelines has been correctly made, of not more than 7 feet.

That the beaches around the head of Lake Michigan give little indication of differential uplift has long been recognized by those at work in the district, as reference to papers by Leverett, Alden, and others given above will show. The gathering of wye-level measurements has simply put the conclusion on a more satisfactory basis than heretofore, and has shown that the horizontal condition is more nearly absolute than might previously have been supposed.

The horizontality of the Algonquin plane is also wholly in harmony with the conclusions reached by Leverett and Taylor several years ago in the case of the beaches of lakes Maumee, Whittlesey, and Warren, around the south and west sides of Lake Erie. For Lake Maumee, Leverett states that

> "west of the Ohio-Pennsylvania line the slight variations displayed by each of its beaches are no greater than may be found along the present shore of Lake Erie; but near the Ohio-Pennsylvania line a differential uplift has caused the beaches to increase perceptibly in altitude in passing eastward."[13]

In the same region, the beach of Lake Whittlesey for a stretch of 200 miles varies less than 15 feet in altitude.

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[13] Leverett: Glacial formations and drainage features of the Erie and Ohio basins. Monograph of the U. S. Geological Survey, vol. 41, 1902, p. 739.

"A part of this difference [of altitude] may be due to discrepancies between the railroad surveys and a part to the difference in height to which the beach was built above mean lake level. These elements of error and of variation being eliminated, it is doubtful if enough difference will remain to require any crust warping." [13]

Similar statements are made by Leverett with regard to the beach of Lake Warren.[14]

The horizontality of the southern portion of a water-plane only 500 miles west of Lake Michigan was strongly suspected, indeed, as long ago as the '80's, when J. E. Todd and Warren Upham secured measurements of altitude of the beaches of glacial lakes Agassiz and Dakota. In his monograph on Lake Agassiz, Upham says, with reference to Todd's data:

"It is evident . . . that the long area of Lake Dakota has experienced only slight differential changes of level, at least in the direction from south to north since the departure of the ice. The James River Valley is thus strongly contrasted with the northern uplifting that has affected the Red River Valley, as shown by the beaches of Lake Agassiz." [15]

De Geer remarked, in 1892:

"As Prof. J. E. Todd and Mr. Upham have stated, the deserted shores of Lake Dakota, situated close to the southwest of Lake Agassiz, show no or only a slight unequal deformation. As the longer axis of this lake trends in nearly the same direction as the greatest warping of Lake Agassiz, it seems probable that the limit for this warping and at the same time for the upheaved area lies just between Lake Agassiz and Lake Dakota, or through Lake Traverse." [16]

THE "HINGE LINE" OR "ISOBASE OF ZERO"

The line which separates the region in which a beach has been upwarped from that in which it is still horizontal has been called by Leverett a "hinge line."[17] Because of its definite implication of warping on one side and stability or uniformity on the other, this term seems preferable to "isobase for zero," which was used by De Geer. The latter term, unless qualified by a more definite phrase, allows the interpretation that on one side of the line there has been differential uplift and on the other

[13] Idem, p. 756.
[14] Idem, p. 765.
[15] Upham: The glacial Lake Agassiz. Monograph of the U. S. Geological Survey, vol. 25, 1895, p. 267.
[16] De Geer: Proceedings of the Boston Society of Natural History, vol. 25, 1892, pp. 471-472.
[17] This term was suggested to Mr. Leverett by the facts in the Erie basin, but has not been used by him in print. It has been used by Coleman in a different sense, to mean a node line drawn from an extinct outlet through points where a beach splits vertically because of uplifts of that outlet. (See Coleman, Bulletin of the Geological Society of America, vol. 15, 1904, p. 363.)

side differential depression. It is interesting to see, however, that De Geer, in employing "isobase for zero," seems to have thought of it as a hinge line. In his remarks on the deformation of the shorelines of lakes Agassiz and Dakota, already quoted, he makes "isobase for zero" equivalent to "the limit for the uplifted region," and although he dwells upon the uplift north of the line, he says nothing to suggest depression south of it. The conception of a "hinge line" is, therefore, no new one.

Upham, De Geer, and others long ago recognized that successive uplifts probably did not hinge on the same line.[18] This appears to be true in a measure in the case of the upwarpings of water-planes of the Lake Michigan basin, for the northern limit of horizontality of the beaches of Lake Chicago is near the latitude of Milwaukee, Wisconsin, and Grand Haven, Michigan, while the corresponding limit for the Algonquin plane is about 100 miles farther north. Measurements of altitude of the plane of the later Nipissing shoreline, however, indicate that all post-Algonquin deformations hinged on a single line.[19]

THE ALGONQUIN PLANE AS A DATUM PLANE

It has been stated that south of the "hinge line" the Algonquin beach varies at most but 17 feet, and probably not more than 7 feet in altitude for 350 miles from east to west and 200 miles from north to south. Although this region may possibly have been raised or depressed from its original position so uniformly as not to warp the beach perceptibly, such a movement seems hardly probable. A variation of at least 20 or 30 feet might be looked for in such long distances. While recognizing the possibility of an even uplift or depression, therefore, we may reasonably favor the idea that in the region of horizontality the Algonquin beach stands today very nearly, if not precisely, where it stood when made.

Since the beach itself in this region has an average altitude of 605-607 feet above sealevel, the calm-water level of Lake Algonquin at this stage may be placed at 600 feet. This figure is open to certain corrections, however, for conditions peculiar to the latter part of the glacial period, chief among which is the lowered sealevel which resulted from the storing up of water in the ice-sheet.[20] According to Upham, the sealevel during the stage of greatest glaciation may have been 100 or 150 feet below its normal position.[21] By the time the ice-sheet had melted back

[18] See passages just referred to, and especially Upham, op. cit., p. 481.
[19] Goldthwait: Journal of Geology, vol. 16, 1908, pp. 473-474.
[20] R. S. Woodward: On the form and position of the sealevel. Bulletin of the U. S. Geological Survey, no. 78, 1888, p. 60.
[21] Upham: op. cit., pp. 515-516.

to the northern border of Lake Algonquin, however, a considerable portion of this water must have been restored to the sea. Hence the correction to be added to the 600-foot altitude of the Algonquin beach is probably much less than 100 feet.

This horizontal plane is a useful plane of reference from which to estimate the amount of post-Algonquin uplift of more northerly districts.[22] The figures 50, 100, 150, etcetera, on the isobases in plate 5 may, therefore, be conceived to indicate not simply the excess of uplift along those lines over uplift south of the "hinge line," but the total amount by which the altitudes of points on those lines have been increased since the time of the Algonquin beach.[23]

The Iroquois Water-plane

Relative ages of the Iroquois beach and the Algonquin beach

The outline of Lake Iroquois in figures 1 and 2 is based on several maps, but especially those of Gilbert,[24] Fairchild,[25] and Coleman.[26] The location of the ice border, however, is wholly hypothetical, and shows only in a rough way the position of the Ontario ice-lobe as a barrier against the northwest side of the Adirondacks. The outlet into the Mohawk Valley at Rome, New York, is also shown.

It has long been thought that Lake Iroquois for at least a part of its existence was a contemporary of Lake Algonquin. The channel of the ancient Algonquin River, apparently connecting the two lakes, first suggested this relation.[27] More particularly, evidence seemed to have been found in a great delta built by the Algonquin River near Peterboro, Ontario, at about the level of the Iroquois Beach.[28] Gilbert, however, remarked that the correlation of this delta with the Iroquois beach was doubtful, and Coleman has more recently stated that the delta was built in a small tributary lake, which he names Lake Peterboro.[29] We can not

[22] The earliest printed statement of this idea is probably this sentence in fine print in Leverett's report on the surface geology of Alcona County, Michigan Geological Survey, Annual Report for 1901, p. 53: "The departure from horizontality here in Michigan may, therefore, be taken as a measure of the amount of uplift that has occurred."

[23] Goldthwait: The attitude of the Algonquin beach and its significance. (Abstract.) Science, new ser., vol. 28, 1908, pp. 382-383.

[24] G. K. Gilbert: The history of Niagara River. Sixth Annual Report of the Commissioners for the State Reservation at Niagara, 1890, pp. 61-84, map, pl. 5.

[25] H. L. Fairchild: Twentieth Annual Report of the New York State Geologist, 1902, pl. 19; and Bulletin of the New York State Museum, no. 127, 1909, pl. 42.

[26] A. P. Coleman: Bulletin of the Geological Society of America, vol. 15, 1904, pl. 22.

[27] Spencer: Proceedings of the American Association for the Advancement of Science, vol. 37, 1888, pp. 198-199.

[28] Gilbert: The Algonquin River. (Abstract.) American Geologist, vol. 18, 1896, p. 231.

[29] Coleman: op. cit., pp. 357-358.

conclude, therefore, that the Peterboro delta and the Iroquois beach are synchronous, and consequently that the Iroquois beach, the Algonquin River, and the Algonquin beach are synchronous. Indeed, Gilbert's observation that the channel of Algonquin River continues down the Trent Valley below the plane of the Iroquois beach to Lake Ontario may be quoted as evidence that Lake Iroquois had already disappeared before the Algonquin River ceased to drain Lake Algonquin;[30] in other words, that the Iroquois beach is of earlier date than the Algonquin beach. On figure 2 the Iroquois shoreline is drawn as a dotted line, because of this evidence that the two lake stages are not strictly synchronous.

The question, therefore, arises: "How much older is the Iroquois beach than the Algonquin beach?" Some light can be thrown on the question by examining and comparing the respective water-planes.

ISOBASES OF THE IROQUOIS PLANE

For plotting isobases of the Iroquois beach, measurements have been selected from those made by Gilbert,[31] Spencer,[32] Coleman,[33] and Fairchild.[34] Of these, Coleman's paper is of peculiar interest because his conclusions concerning the attitude of the Iroquois plane are matched very closely by those since reached for the Algonquin plane. The measurements are as follows:

	Feet	
Hamilton	363*	Spencer.
Waterdown	365*	Spencer.
Cooksville	400	Spencer.
Toronto Junction	422–425*	Coleman.
York	442*	Coleman.
Kingston Road	459*	Spencer.
Whitby	507*	Spencer.

[30] Gilbert: loc. cit.

[31] G. K. Gilbert: Old shoreline of Lake Ontario. (Abstract.) Science, vol. 6, 1885, p. 222.

Old shorelines in the Ontario basin. Proceedings of the Canadian Institute, 3d ser., vol. 6, 1888, pp. 2-4.

[32] J. W. Spencer: Deformation of the Iroquois beach and birth of Lake Ontario. American Journal of Science, 3d ser., vol. 40, 1890, pp. 443-451.

On the focus of regional post-glacial uplift. Transactions of the Royal Society of Canada, sec. 4, vol. 7, 1889, p. 129.

Evolution of the falls of Niagara. Geological Survey of Canada, 1907, pp. 203-204, 208, 277-284.

[33] A. F. Coleman: op. cit., especially pp. 358 and 359-362.

[34] H. L. Fairchild: Pleistocene geology of western New York; report of progress for 1900. Twentieth Annual Report of the New York State Geologist, 1902, pp. 103-139.

Gilbert Gulf (marine waters in the Ontario basin). Bulletin of the Geological Society of America, vol. 17, 1906, pp. 712-718.

Glacial waters in central New York. Bulletin of the New York State Museum, no. 127, 1909, pp. 54-55.

The measurements at Albion, Rochester, Auburn, Sodus, Rome, and Farrs were supplied by Professor Fairchild from his unpublished data.

	Feet	
Quays Siding	557	Coleman.
Colborne	602*	Spencer.
Trenton	632*	Coleman.
Queenstown	385*	Spencer.
Albion	430	Fairchild.
Rochester	440	Fairchild.
Sodus	456	Fairchild.
Auburn	411	Fairchild.
Canastota	441	Gilbert.
Rome	460	Fairchild.
Cleveland	484	Gilbert.
Constantia	489	Gilbert.
Richland	566	Fairchild.
Adams	640	Fairchild.
Adams Center	657	Gilbert.
Brookside Cemetery	700	Fairchild.
Farrs (east of Watertown)	733	Fairchild.

* These are wye-level measurements.

These measurements have been plotted at the respective localities on plate 5, and isobases have been drawn across Lake Ontario with reference to them. It will be noticed that the altitude of the Iroquois beach at York (442 feet) is nearly equal to that at Rochester (440 feet) and that at Canastota (441 feet). The isobase drawn nearly through these localities, therefore, serves to fix the probable direction of tilt at that altitude. It indicates what Fairchild and other writers have remarked, that the direction of tilt is more nearly due southward over the east end of Lake Ontario than over the west end. The same feature is met also in drawing isobases from Quays Siding (557 feet) to Richland (566 feet) and from Trenton (632 feet) to Adams (640 feet).

Coleman found that the direction of inclination of the Iroquois plane over the western part of Lake Ontario is about south 20 degrees west.[35] The isobases on plate 5 agree closely with his statement, running about 22 degrees south of east.

Comparison of the Two Water-planes

A comparison of the Iroquois isobases with the Algonquin isobases in southwestern Ontario brings out remarkable similarities of attitude in the two planes. The direction of tilt, as we have seen, is within one or two degrees of south 20 degrees west in both cases. This obviously means that the two beaches have been deformed by the same set of movements.

[35] Coleman: op. cit., p. 360.

A still more remarkable resemblance between the Iroquois and Algonquin planes is found when the tilt rates of the two are compared. It is a matter of some convenience that the measurements available on the Iroquois beach are so distributed that they afford the construction of a set of isobases at 100-foot intervals, if we take for our zero the altitude of the beach at its southernmost point, Hamilton (363 feet). This has been done in figure 3. It is well recognized that the differential uplifts which warped the Iroquois plane extended southward as far at least as the head of that lake, at Hamilton, and an unknown distance beyond. Hence, although we take a line through Hamilton (in the direction 22 degrees south of east) as a zero line, we must not forget that zero is not a true measure of the amount of differential uplift at that place. It is quite probable that the post-Iroquois uplift at Hamilton amounts to 15 or 20 feet. We may therefore call the line a "zero plus" line, and the others "100 plus," "300 plus," etcetera. Having done this (see plate 5), we may examine the tilt rates between these isobases and compare them with the tilt rates between corresponding isobases of the Algonquin plane in the adjoining district.

Table for the Comparison of Tilt Rates of the Iroquois and Algonquin Planes

Portion of plane represented (isobases).	Iroquois plane.		Algonquin plane.	
	Data.	Tilt rate (feet per mile)	Data.	Tilt rate (feet per mile).
0 to 100	Hamilton (363 feet) to Kingston road (459 feet), 42 miles.	2 24	Grand Bend (607 feet) to Port Elgin (710 feet), 79 miles.	1.30
100 to 200	Kingston road (459 feet) to Quays (557 feet), 32 miles.	3.06	Port Elgin (710 feet) to Owen Sound (748 feet); and Holland Landing (752 feet) to Oro (811 feet), 40 miles.	2.43
200 to 275	Quays (557 feet) to Trenton (632 feet), 18 miles.	4.17	Oro (811 feet) to Kirkfield (883 feet), 18 miles.	4.00

The comparison shows in each case a somewhat steeper tilt for the Iroquis plane than for the Algonquin. This must be due in a measure to the fact that the zero isobase for the Algonquin plane marks the real limit of uplift, while the "zero plus" line for the Iroquois plane is probably a number of miles north of the true "hinge line." If we only know how far south of Hamilton to move the "zero plus" line to put it in the

right position for a "hinge line," we might move the "100 plus," "200 plus," and "300 plus" lines southward to correspond, and we could then estimate the tilt rates more correctly. All these rates would, of course, be smaller after this correction than they appear in the table, because of the southward flattening of the plane; but the rate nearest the zero line would be affected most. The result of decreasing the tilt rates of the Iroquois plane in the table and of decreasing especially the rate of 2.24 feet per mile for the stretch between the 0 and 100 isobases, would be to make these tilt rates agree even more closely than before with the rates for corresponding stretches of the Algonquin plane.

The bearing of this similarity of attitude of the Iroquois and Algonquin water-planes upon the question of their relative ages is obvious. If, as Gilbert's observations seem to require, the Iroquois beach is older than the Algonquin, it can not be much older, for it has been tilted only a little more than the Algonquin. Uplifts are known to have been going on for some time previous to the two outlet stages of Lake Algonquin, and to have continued for some time after it. Yet the Iroquois beach seems scarcely to have been affected by those uplifts which preceded the two-outlet stage. The two beaches appear, therefore, to be almost synchronous. In other words, it appears that the Ontario ice-lobe had only recently withdrawn from the northwest slope of the Adirondacks when the Algonquin River ceased running and the Algonquin beach began to rise above the 600-foot plane.

The Isobases and the pre-Cambrian Boundary

In his paper on "Quaternary changes of level in Scandinavia,"[36] in 1891, De Geer said:

"The isanabases of Sweden were found to conform with the limits of the Scandinavian Azoic territory, and, according to the very latest determinations, not only in a general way, but also in many details. . . . The coincidence between the area of upheaval and the Azoic territory may possibly be explained by assuming that this territory, which is an old tract of erosion, has also been one of continental upheaval, which subsided during the ice age, for the greater part perhaps in consequence of the considerable ice-load, again rising after the release from the latter, though not to its former altitude."

Again, in his paper of the following year,[37] De Geer emphasized

"the coincidence of the uplifted area with the Scandinavian Azoic region, or what Suess has called 'the Baltic shield' . . . a well-defined tract where

[36] De Geer: Bulletin of the Geological Society of America, vol. 3, 1892, pp. 65-66.
[37] De Geer: On Pleistocene changes of level in eastern North America. Proceedings of the Boston Society of Natural History, vol. 25, 1892, pp. 454-477.

the old rocks are laid bare by erosion and the surrounding lands thickly covered with younger sediment. The limit of the Baltic shield, where it has been directly observed, and perhaps everywhere, is marked by great faults. Now the isobase for zero, or the boundary for the uplifted area, seems all the way a little outside of the above-named limit, and follows very conspicuously its convexities and concavities. Likewise all the other isobases point to a close connection between the upheaval and the geological . . . structure of the land."[28]

In the same paper, after reviewing the evidence then available for North America, De Geer writes:

"The conformity between ice load and subsidence seems to have been still greater here than in Scandinavia; and in this respect it will be very interesting to see what will result from a continued investigation of the warped beaches in the lake basin with its marked ice-lobes. . . . The connection between the subsidence and the geological structure of the earth's crust is perhaps not quite so striking as in Scandinavia. Still it seems probable that the Canadian Azoic or Archean region has changed its level more than the surrounding tracts. . . . The general conformity between the ice covering and the old Azoic plateau makes it difficult, in the present state of our knowledge, in many cases to discern between the influence of these two circumstances."[29]

The question thus raised is too difficult to be answered satisfactorily even yet, because too little is known of the course of isobases outside of the Michigan-Huron-Ontario basins. An examination of the isobases for even this limited portion of the Great Lake region, however, is not without interest.

In the first place, the recent detailed studies of the Algonquin plane have indicated in every district that local irregularities, if present, are immeasurably small. The uplift around such depressions as Green Bay, Wisconsin; Grand Traverse Bay and Saginaw Bay, Michigan, and Georgian Bay, Ontario, show accordance—not discordance—with the broader, deeper lake basins adjoining them. The wye-level surveys have discovered no looping of the Algonquin isobases where they cross the lakes, such as one might expect from the greater thickness of the ice-lobes in those basins which are bordered by concentric moraines. If the melting away of the ice-sheet was the immediate cause of the uplifts of the region, the irregular load of these late, lobate stages must have been a very small factor in the equation, perhaps because the ice-sheet at that time possessed but a small fraction of its original thickness. The isobases run in a general way parallel to the boundary of maximum glaciation, but they

[28] De Geer: op. cit., pp. 458-459.
[29] Idem, pp. 473-474.

do not appear to conform at all to the more strongly lobate boundaries of later stages.

Comparing the Algonquin and Iroquois isobases with the pre-Cambrian boundary of Ontario (marked on plate 5 by a line of small crosses), we find a general parallelism. The pre-Cambrian boundary, to be sure, is much more irregular than the isobases; but some of its sinuosities may be ignored, since they indicate the presence merely of thin outliers of the sediments or of protruding inliers of the crystallines. The direction of the border of the crystallines seems not to depart for any considerable distance or to any considerable degree from the direction of the isobases.

Whether the Adirondack oldland in the east and the pre-Cambrian area of northern Wisconsin in the west were also central areas of uplift or not, around which the isobases of the Iroquois and Algonquin planes if extended would turn, is a question which can not be answered from data now at hand. Such measurements as are available give no indication of it.

In agreement with De Geer's statement, therefore, it may be said that the parallelism of isobases to the border of the pre-Cambrian oldland in Ontario is as suggestive as the parallelism of isobases to the border of the glaciated area; that a causal connection between the area of long-continued erosion, the area of glaciation, and the area of post-glacial uplift may fairly be inferred from the most recently collected facts.

SUMMARY

Detailed surveys of the Algonquin beach around Lake Michigan, Lake Huron, and Georgian Bay have recently yielded a body of accurate data from which a set of isobases can be constructed on the upwarped plane of the Algonquin beach at intervals of 50 feet. Incidentally a datum plane appears to have been established, from which one can estimate the amount of uplift in more northerly parts of the Great Lakes region since Algonquin time.

Fewer measurements of altitude of the Iroquois beach around Lake Ontario furnish ground for the construction of a set of isobases at 100-foot intervals on the Iroquois plane.

The similarity between the two sets of isobases is remarkable, both as regards the direction and the amount of tilt indicated for corresponding parts of the planes. Accepting Gilbert's conclusion from physiographic evidence that the Iroquois beach is of earlier date than the Algonquin, we are led by this comparison of the two tilted planes to conclude that the difference in age between them is probably very slight.

The rough parallelism of the isobases with the glacial boundary on the one hand, and with the border of the pre-Cambrian oldland of Canada on the other, in connection with the marked increase of tilt rate near the latter boundary, seems to strengthen the position taken by De Geer[40] twenty years ago, that in North America, as in Scandinavia, the central areas of the recent upwarpings were not merely centers of glaciation, but centers of continental uplifts of much earlier dates.

[40] In his view regarding the central area of uplift in Canada, De Geer followed Spencer, who as early as 1889 attempted to locate the precise center of post-Algonquin upwarpings in a paper "On the focus of regional post-Glacial uplift" (Transactions of the Royal Society of Canada, section 4, vol. 7, 1889, p. 129). Spencer reached the conclusion that the center or focus was on the height of land southeast of James' Bay. De Geer's isobases are concentric with that point. In view of the subsequent discovery of the far westward trend of the isobases of Lake Algonquin and Lake Agassiz, it now appears that if the uplifts centered around any one point, that point was much farther north than Doctor Spencer supposed, or that the uplifts centered along an axis which trends westward from his "focus."

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CACOPS, DESMOSPONDYLUS; NEW GENERA OF PERMIAN VERTEBRATES[1]

BY S. W. WILLISTON

(Presented in abstract before the Paleontological Society Dec. 30, 1909)

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[1] Manuscript received by the Secretary of the Geological Society April 23, 1910.

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Introduction

Character of the Permian beds of northern Texas

In a field experience dating from July, 1874, in nearly every known vertebrate horizon of North America, I know of no beds so difficult of exploitation as those of the Permian of northern Texas. As a rule the vertebrate fossils are found more or less broken and weathered, scattered about on the sloping exposed surfaces. Occurring as the bones do, almost invariably concealed beneath a thin or thick covering of cemented matrix, so like the color and form of the countless millions of nodules, it is only when they have been broken up and weathered by exposure that the slight colorational differences presented by the fractured surfaces distinguish them. It thus happens that by far the larger part of the material secured, especially that of the smaller animals, is more or less fragmentary in character, though fortunately preserved from rapid disintegration by the covering of more or less intractable matrix.

Conditions of fossilization

The middle portion of the deposits is to a large extent free from nodular concretions, presenting smooth, clean surfaces of red clay, especially inviting to the collector. But, unfortunately, it is these attractive deposits which are the most barren of fossils. I have searched patiently over such beds for many successive hours without being rewarded by a single fragment of a fossil; and it is for this reason that one, after a little experience, learns to avoid these clay deposits, or gives them but scant attention. Nevertheless it is in these "barren" deposits that the prizes of the Texas Permian are to be sought. When fossils do occur in them, they usually are excellently preserved and largely free from the exasperating matrix. Professor Case has observed that but one or two specimens may be expected from these clay beds in a season's work, and

he is not far wrong in his estimate. And it was one of these prizes which was discovered by Mr. Paul Miller, of the University of Chicago Expedition of 1909, between the valley of Indian Creek and the Wichita River, about 5 miles west of the Vernon road. A large quantity of small bones weathered out and lying in a small gully led to the detection of a bone bed or "quarry" that is without equal in the history of the Texas Permian exploration. The clay lying over the mass of bones protruding from the hillside was entirely free from nodular masses, and, with the exception of a few ounces of bone fragments a half mile away, not another indication of a fossil was discovered in the adjacent exposures covering several hundred acres. The clay was excavated down to the level of the bones, and the bones were then removed in large clay blocks securely bandaged. It was found quite inexpedient to attempt the removal of the bones from the matrix in the field. Not only was the thin adherent matrical covering of the bones so perfectly like the clay in appearance that to distinguish the bones from fragments of the indurated clay when loosened required the most minute examination, but the interlacing of the numerous skeletons in the mass prohibited the attempt to separate them in the field. Indeed, in the laboratory the only safe way to prevent the loss of small bones and fragments of bones when dissociated is to dissolve the clay in water. As it was, with the inadequate tools at our command near the close of the season, the complete excavation of the bed was not attempted, but will be resumed the present season by Mr. Miller, when it is confidently expected that much additional material will be secured.

The skeletons lay in a narrow deposit about 5 feet in width and of unknown extent throughout a thickness of about 2 feet, and apparently on a sloping horizon. They are almost invariably found lying on the ventral side, and for the most part with the members undisturbed, save that in some cases the phalanges of the feet are more or less scattered about, and the tail or whole limbs may be dislodged. Many of the bones have a very thin layer of cemented clay covering them; others are more or less cemented together in nodular masses. It is very evident from the character of the deposits and the position of the bones that the skeletons had suffered but little disturbance after death; that the creatures had for the most part died peacefully in a stagnant, perennial pool of water, successive generations piled upon each other in layers.

I estimate that, so far, there is evidence of at least 50 or 60 skeletons in the deposit. From the loose material recovered from the surface I count over 50 femora, nearly as many humeri and corresponding girdles, with large quantities of the vertebræ. In the blocks containing the thickly aggregated skeletons secured, there are probably representatives

of at least as many more, judging by the two or three blocks that have been so far worked out. How many more remain to be secured the present season I can not even conjecture. We anticipate as much more material as we already have.

ASSOCIATED VERTEBRATES

Not many different kinds of animals are represented by the remains so far examined. The great majority of the skeletons belong to the genus *Varanosaurus* Broili. Another genus of reptiles, very clearly new to science and of about the same size as *Varanosaurus*—that is, 4 feet in length—is represented by fewer remains, but still sufficient, it is confidently believed, to furnish the complete structure of the creature. So far, of this form, the complete hind legs and feet, pelvic girdle, and tail have been worked out, and two skeletons are running into the block matrices. Its restoration and description will be published as soon as possible. A third genus of reptiles, also new to science, represented so far by a femur, which is described and figured (plate 16, figure 5) in the present paper as *Desmospondylus,* was also found. All the amphibian remains so far obtained belong to the present genus *Cacops*, and consist of the nearly complete skeleton shown in the restoration in plate 17, together with another skeleton less complete, portions of two other skeletons and skulls, and various remains of less moment found in the topmost layers. I doubt not that the present material on hand will disclose additional skeletons of this genus more or less complete. One skeleton of *Varanosaurus*, complete from the tip of the tail to the back of the skull, has been completely worked out and figured in 20 plates. An additional skull of another skeleton still leaves some details to be determined, doubtless to be found in other skeletons and skulls yet in the matrix. The full description, with figures and plates of the skeleton of this form, will follow the present paper within a year.

The skeleton of *Cacops*, as shown in plate 17, is composed exclusively of one specimen, with no more plaster in its construction than was necessary to cement the freshly broken parts in its preparation, save of many of the phalanges, as is described further on. The specimen was not only prepared by Mr. Miller, but was skilfully mounted by him in the course of a couple of months' labor. It is doubtful whether another type of fossil vertebrate animal of the size of this is represented by as complete material as is *Cacops aspidephorus.*

Cacops aspidephorus—Genus and Species new

SKULL

(Plates 6, 7, 8)

Two skulls, nearly complete, and portions of two others have so far been recovered from the matrix. Of these the one in the mounted skeleton was quite complete, but suffered slightly at the front extremity in its collection. The best specimen, however, the one from which the following description has been drawn, was a skull quite complete, attached to another skeleton, from which the posterior end of the mandibles only was lost in collecting. This skull is slightly smaller than the mounted one and had suffered very little distortion. It has been freed from every particle of matrix, even that of the brain and nasal cavities. Unfortunately, in none of the specimens has it been possible to determine the sutures, in part because of the complete ossification of the bones; in part, perhaps, because the removal of the thin investing matrix has obliterated whatever indications of them might have been present. The dermal surface is everywhere rugose, with small, irregular pits and ridges.

The skull is broad and depressed, broadest opposite the posterior part of the orbits, with a gentle, perhaps somewhat irregular, convexity on the sides. The epiotics project backward strongly, leaving a deep concavity in the middle behind. The nares are large, oval in outline, directed upward, forward, and outward, broadly separated in the middle, and approaching closely the margin of the maxillæ. The orbits are large, subcircular in outline, the opening looking obliquely upward. Near the middle of their front margin the border is angularly thickened, descending in a steep declivity outwardly. In the middle posteriorly, also, there is a similar, but more angular, thickening, which extends back as a ridge to form the lateral margin of the cranial table, overhanging the otic cavity posteriorly. Almost continuous with the upper orbital margin, there is another elevated ridge running backward and outward to join the lateral border over the middle of the otic cavity. Between these two ridges there is a triangular space of considerable size, more or less depressed in its middle. The least distance between the orbits, near their middle, is but little or no more than one-half the lateral diameter of the orbit. The large parietal opening is located about one-third of the distance between the hind borders of the orbits and the occipital margin in the middle. Just in front of the concave hind border of the table the margin is elevated into a prominent rugose crest or ridge, highest in the middle, possibly for the attachment of nuchal muscles. The cranial table in front of

this transverse rugosity, and between the slightly elevated lateral margins, is flat or gently concave.

On the sides posteriorly there is a large cavity, with an angulated slitlike opening at its bottom, forming a false temporal fenestra, which doubtless is merely a greatly enlarged and closed otic notch.[2] It is bounded above by the heavy overhanging lateral border of the cranial table. The epiotic angles, produced into long horns, instead of ending freely, as in other Stegocephala, turn directly downward to fuse with the quadrate below, inclosing what would otherwise be a simple notch into a large fossa and opening. Its whole exterior and outer surface is roughened like other parts of the skull. The cavity thus inclosed extends angularly at its upper anterior angle to within about 20 millimeters of the middle of the hind border of the orbit and is smooth throughout. At its bottom there is a thin, flat, angular plate, attached to the lower anterior inner side of each epiotic horn, projecting upward and forward to an acute angle, leaving a narrow, slitlike perforation above connected with another in front reaching the lower part of the cavity, angularly dilated at the upper anterior part. Close to the anterior border of this plate, and near its upper angle, is the projecting end of the stapes, as shown in plate 8, figure 1. The upper margin of the perforation in front is formed by a narrow descending plate from the rugose upper border of the cavity. The front wall of the cavity slopes backward from the upper angle to a little above the quadrate articular surface; its smooth wall looks obliquely upward, backward, and outward. It seems probable that this cavity, as thus bounded, was closed by a tympanic membrane, against which the continuation of the stapes abutted.

The precise limits of the epiotic process are not certain, but a distinct line is evident on one side, indicating sutural attachment with the quadrate along the posterior side of the platelike expansion and to within a short distance of the articular projection. This, I am aware, is an unusual position for the quadrate, with the ear-slit or opening above and in front of it, but there can be no other interpretation of the structure. The quadrate is well ossified below, fused with the extremity of the pterygoid on the inner side and with the quadrato-jugal in front below.

The occipital surface of the skull has, in the middle, a smooth, steep declivity, with the small foramen magnum at the bottom, not more than 5 millimeters in diameter. The high rugosity of the posterior border of the cranial table overhangs slightly this declivity, forming a fossa into which doubtless were inserted the strong neck muscles. Just outside of

[2] A closed otic notch is not unknown among Stegocephala. See Woodward, Proceedings of the Zoological Society, 1904, p. 170, plate xi. *Capitosaurus stantonensis* Wood.

each condyle there are the usual two cranial foramina at the base of the paroccipital processes, which extend outward, joining the epiotic on the under side and turning downward to terminate at or near the upper posterior corner of the quadrate. Between this paroccipital process and the roof, at each side, there is a moderately large post-temporal vacuity leading into a deep cavity just inside the "tympanic" rim of the ear cavity and below the roof of the skull.

The structure of the under side of the skull, while not departing far from the usual stegocephalan type, is somewhat remarkable. The palatal cavity as a whole had a high arched roof, with a slender, almost vestigial, parasphenoid dividing the large pterygoidal vacuities so characteristic of the amphibia. The internal nares are larger than the external ones, situated either side close to the teeth, with their front margin almost below the hind margin of the external nares. On the inner side of each there is a stout, conical, recurved tooth, about 10 or 12 millimeters in length, and another like it is situated near the posterior margin of the orifice, not far from the maxillary teeth. These are the only teeth located on the palatal surface. The palatines, or conjoined palatines and pterygoids, on each side posteriorly and internally to the nares slope strongly upward. In the middle in front the vomers or anterior end of the parasphenoid is about 15 millimeters in width where it joins the palatine shelf, narrowing to about 4 where it joins the rhinencephalic chamber. Posteriorly the arrangement of the basicranial bones is very similar to that of *Trematops* or *Eryops*. The ossified basisphenoid sends downward and outward a stout basipterygoid process to join the pterygoids on each side, the juncture indicated by a thickening as in *Trematops*. Outwardly the pterygoids form a vertical plate, probably with the conjoined epipterygoids, reaching to the cranial wall, save for a small vacuity near the roof, leading into the deep temporal cavity under the roof, into which the post-temporal vacuity also opens, a cavity open below back of the pterygoids between the basicranial bones and the posterior wings of the pterygoids. In the middle the parasphenoid continues forward from the basisphenoid as a slender rodlike bone nearly to the flat anterior end. For a short distance it forms a high bridge, above which in front is the inferior opening of the parietal foramen. For the larger part of its distance, however, it is closely united as a thick ridge to the lower side of the elongated rhinencephalic chamber (see plate 7 and plate 8, figure 1). Between this chamber and the closed brain-chamber behind there is a large orifice on each side for the escape of the optic nerves. This elongated arched chamber has its anterior borders also free and curved a little back of the anterior margin of the orbits. In front of this the roof of

the skull has been somewhat pressed down upon the palatal bones, but there was evidently in life a high cavity for the nasal region, in which are various indeterminate bone remains, doubtless the ethmoidal and turbinate. A little in front of the basisphenoid the pterygoids give off a rounded or subangular process much as in the allied forms, narrowing the opening of the infratemporal fossa, which is broad and deep behind, where its thin upper posterior roof forms the anterior inferior wall of the otic cavity.

The basisphenoid is concave in the middle; on either side it has a flattened basisphenoid process, as in *Trematops*, directed downward and backward, underarching a rather deep fossa. Opposite these on either side is the root or base of the pointed, stylelike stapes, which is directed outward and backward to terminate, as already described, at the upper angle of the quadrate, in the auditory vacuities. Whether or not it has a foramen at its base I can not say. Above and in front of this, turned upward to reach nearly to the inner surface of the superior tympanic ridge, is the proötic bone.

A comparison of the structure of the basicranial region with that of *Trematops* shows great similarity, quite confirming my suggestion that the pseudotemporal vacuity is in reality merely the closed otic notch for the opening of the external ear. The opening in *Trematops*, however, is far smaller than in *Cacops*, and extends somewhat further forward toward the orbit. The small size of the parasphenoid in the present genus also explains its apparently entire absence in *Trematops*, though it is not improbable that the rhinencephalic canal in *Trematops*, were it preserved complete in the type specimen, would show the remains of the parasphenoid coalesced with it as in *Cacops*.

The maxillary teeth in *Cacops* are all small and of nearly uniform size, in an uniform closed series. I count about 20, but it is possible there may have been a few more.

The mandible is remarkable for its slenderness. Posteriorly it has a broad expansion, but the ramus from the middle of the orbits forward is slender. A deep fossa is present in front of the condyle, and the median symphysis in front is a little expanded. It has apparently the same number of teeth as in the maxillæ, and all, like them, are of uniform size.

VERTEBRÆ

(Plate 9)

The vertebral column of the mounted skeleton was apparently quite complete as it lay in the matrix in association with skull and limbs. In

the removal of the thin incrusting matrix, however, a few of the pleurocentra of the anterior vertebræ and some of the small elements of the tail were lost, notwithstanding the most scrupulous care. Of these, only the possible pleurocentra of the atlas and the dorsal elements of the first 5 or 6 caudal vertebræ have any morphological significance; future preparations of other skeletons yet contained in the matrix will doubtless complete even these small details. The column as found was continuous from the skull to the tip of the tail, without break, save that the last few caudal vertebræ were slightly angulated from the rest of the series. The presacral vertebræ had a gentle, sinuous curve, with the convexity to the right as far back as the end of the dermal carapace, to the left from thence to the sacrum. There is also a slight vertical sinuosity in the same regions, convex above anteriorly, below posteriorly. These curvatures seemed so normal that no attempt has been made to reduce them in the mounted skeleton, and I have figured the column as it lay. A slight pressure to the left has crowded the ribs upward on that side and downward on the opposite side, but to a very slight extent only. The perfect union of the different elements, at least as far as the beginning of the chevron caudal vertebræ, removes all possible doubt as to their number and relations—21 presacral, 2 sacral, 6 pygals, and 15 or 16 chevron caudals. The spines, save those of the first 2 vertebræ, are of nearly uniform length throughout the carapacial series, 15 in number, a trifle longer perhaps in the anterior and middle region, and a little more slender in the last 3 or 4. Those of the free presacral vertebræ are progressively shorter and less stout. Throughout the series covered by the carapace they are slightly thickened at the upper end, with the anterior and posterior margins thinned, and with a lateral ribbed thickening on each side near the middle, as though for the support of the terminal expansion. Covering the top of each spine there is a rooflike expansion, wider in the middle and narrowed at each lateral end. Their sides slope downward at an angle of about 45 degrees to the full width of the superincumbent dermal scutes. The anterior margin of these plates is uniformly beveled for articulation with the posterior margin of the superincumbent intercalated dermal scute. The posterior beveling is much broader in the middle. Presumably these expansions are outgrowths from the top of the spine, cartilaginous in origin, but of this I do not feel entirely assured, since in every case where I have removed them I have found what appears to be a sutural surface, and the top of the spines is rounded on the margin, with an extrant angle between it and the plate. The surface of these expansions is smooth, both above and below. The narrowed outer extremities are either rounded or with a slight emargination. The first

of these expansions, that of the second vertebra, is small, subtriangular in shape, with rounded corners, and appears not to have been covered by a dermal scute. The second expansion is larger, becoming broader behind, and is covered, on its posterior part only, by the first dermal scute. The posterior end of the carapacial series tapers more gradually than does the anterior to a narrower extremity, and like that has, apparently, no dermal plate over the last of the series, the penultimate spinal expansion supporting the posterior margin of the last dermal shield. Throughout this series the thinned expansions of the spines above, anteriorly and posteriorly, touch each other in the present curved condition of the column. In the most anterior part of the column, however, the hypocentra are slightly separated, with one longer interval, producing a slight convexity of the series below. It thus seems certain that the position in which the column was found, and which has been retained in the mounted specimen, was a normal one for the living animal; that is, with a gentle convexity antero-posteriorly of the carapace, and a slight concavity below. Nor would it have been possible for the living animal to have fully straightened out the column without actually dislocating the zygapophysial articulations. A slight lateral bending was possible in life, as shown by the position in which the bones were found; but even this could not have been extensive in the front part, since the free dermal plates which glided smoothly over the fixed spinous expansions would have met each other at their lateral extremities, if the curvature was at all decided. Back of the carapace, however, a greater flexibility was possible, since the zygapophyses here are somewhat larger, and the free spines were separated above by a greater interval.

CARAPACE

(Plate 9 and plate 12, figures 5, 6)

The dermal plates are of nearly uniform length, increasing slightly in expanse to the middle of the series—that is, at the summit of the dorsal convexity. Each fits accurately and closely over the contiguous borders of the adjacent spine roofs, separated from each other by a space of 1 or 2 millimeters. It is evident, from the structure of the spine expansions with the greater beveling in the middle behind, that the chief motion was at the anterior part of each dermal plate. The upper surface of these plates is slightly irregular, with shallow depressions or pits, the margins in front and behind parallel, with a slight obliquity backward; their outer angles are slightly rounded, and their outer, thin borders are nearly straight, or with a slight emargination. Each of these dermal plates is composed of two elements, a median longitudinal suture being evident in

many of the shields. In the middle of each above there is a shallow groove bordered on each side by a slight elevation.

In the structure of the dorsal shield or carapace the genus is identical with *Dissorophus*, save that the carapace of the latter genus is very much more extensive, covering practically the whole of the dorsal side of the animal and probably extending further back. Furthermore, in *Dissorophus* the anterior shield is very large, covering several vertebræ, very much like the anterior shield in certain armadillos. That these differences in the carapace, together with others of the skeleton, as shown in an excellent specimen of *Dissorophus* in the University of Chicago collections, which will be fully figured later, are not due to age, is evident from the close correspondence in all skeletal characters between several specimens found associated in the present matrix. These differences will be summarized in the taxonomic discussion on a later page.

CARAPACE IN ALLIED FORMS

Most remarkably similar are the dorsal shields of the genus *Aspidosaurus* Broili, which in other respects, so far as the skeletal characters are known in the rather fragmentary type, resembles so closely the present form that I was for a long time almost assured of their identity, and this opinion was also shared by Doctor Broili, to whom I sent for comparison photographs of the drawings of the plates illustrating the present paper. But a careful study, not only of the figures and descriptions given by Broili, but also of a fragmentary specimen in our present collections, which must be identified as conspecific with the genotype of *Aspidosaurus*, quite convinces me, as it has also Doctor Broili, of the generic distinction between the two forms.

In *Aspidosaurus* there is but a single dermal expansion over each spine, apparently coössified or continuous with the top of the spine, which overlaps the preceding one like shingles on a roof. Or, as Broili has stated it:

> "Diese Hautverknöckerungen treten nun in innige gegenseitige Verbindungen insofern sich dieselben aufeinanderliegen und zwar immer die folgenden auf die vorausgehende Ossification, wodurch das Gänze gewisse Aehnlichkeit mit dem First eines Hauses gewinnt. Diese eigenthümliche Schutzapparat fällt noch besonders durch seine Skulptur auf, welche die nämliche kräftige Ausbildung zeigt, knötchenartige durch Leistchen verbundene Auschwellungen, wie der Schädel. Die Unterseiten dieser Hautverknöckerungen sind glatt, nur ihrer seitlichen Ränder weisen leichte Einkerbungen auf."[1]

Of the specimen above mentioned I give figures (plate 10, figures 2, *a*, *b*) showing this very characteristic difference. It is seen that the broad

[1] Paleontographica, vol. II, 1904, p. 42.

spinous expansions overlap like shingles the ones preceding. The roof also differs in the greater angularity of the ridge, the sides sloping at a greater angle, while in *Cacops* there is a median depression or shallow groove, and the lateral plates are more convex. The spines supporting these plates are much more expanded above than is the case in *Cacops*. From Broili's figure of the type of *Aspidosaurus* it is not certain that the otic notch is inclosed by bone, as in the Dissorophidæ, though the emargination shown would suggest that such may be the case.

A specimen recently described by Case[4] and referred doubtfully to *Zatrachys apicalis* is in all probability a species of *Aspidosaurus*, a genus apparently overlooked by the author. The author described the carapace as having overlapping plates, with great angularity, apparently quite as in the specimen herewith figured, and as in the type specimen figured by Broili. If the specimen be conspecific, or at least congeneric with *Aspidosaurus*, as I doubt not, then an additional generic character is furnished by the ribs. These have an elongate hooklike process directed upward and backward over the succeeding ribs, quite unlike the structure in *Cacops*.

Altogether the differences presented by *Aspidosaurus* are so fundamental that I believe Broili was justified in separating the genus from *Dissorophus* on the character of the carapace alone. If so, then certainly *Cacops* can not be united with *Aspidosaurus*, even though the size and extent of the carapacial development is much more like that of this genus than of *Dissorophus*. Possibly the *Aspidosaurus* type of roof has been derived from that characteristic of the Dissorophidæ by the anchylosis of the dermal scutes with the superior expansions of the spines, but even such a difference as this is of generic importance.

Other genera of temnospondyles are known to possess similar carapacial developments. *Euchirosaurus*, from the Lower Permian of France, has a broad dilatation of the spines above, though not platelike. Very similarly expanded spines I have seen in specimens from the Texas Permian, but have been inclined to refer them to the basal caudal region of *Eryops*.

The structure of the dorsal expansion of *Zatrachys* is very imperfectly known, unless, indeed, Case was right in referring his species *apicalis* to that genus. The vertebræ are unknown in the type species. In another species, referred to the genus by Cope later, the real *Z. apicalis*, he described the spines as follows: "The summits of the neural spines are expanded and the superior faces of the expansion are tubercular and have

[4] Bulletin of the American Museum of Natural History, vol. xxiii, 1907, p. 665.

a median prominence. The expansions are sometimes large, resembling the dermal bones of the crocodiles, and in that case the median prominence is a keel. On the smaller expansions the latter is a mere apex. There are narrow, flat bones which I suppose to be neural spines, which are ornamented with innosculating ridges."[5] These characters agree better with a spine figured and described by Case as *Zatrachys crucifer* Case,[6] the type of which I have examined. This is of much larger size, and is in the shape of a cross, the median projecting piece above and the upper surface of the transverse arms are deeply pitted—a radically different type from that of either *Aspidosaurus* or *Cacops*. The description of the skull seems to preclude the possibility of the genotype being congeneric with *Cacops aspidephorus*.

HYPOCENTRA

(Plate 9, plate 12, figure 7, and plate 14, figure 12)

The hypocentra, like the arches, are of nearly equal size and extent throughout, rather strongly and smoothly convex from side to side, gently concave antero-posteriorly. The second to the eighth back of the atlas have, on each side, near the upper angle posteriorly, a facet or protuberance for the capitular articulation of the rib. The first hypocentrum back of the atlas is smaller than the succeeding ones; it is shorter and has more acute lateral margins, and is, apparently, without facets for the capitular articulations. The second hypocentrum is also somewhat smaller than the succeeding ones, but is provided with a parapophysial protuberance on each side.

PLEUROCENTRA

(Plate 9 and plate 12, figure 5)

Of the first four vertebræ back of the atlas the pleurocentra were not recovered; they doubtless had dropped out of their places and were not recognized in the matrix, since a slight depression of this part of the column had loosened all the elements somewhat. However, in the connected series places for them are shown with articulations indicating but little variation in size from that of the following ones. In size the pleurocentra of the remaining vertebræ are nearly uniform, perhaps slightly longer anteriorly than posteriorly. Each articulates broadly with the posterior side of the pedicel of the neurocentrum, and, less extensively, with the anterior side of the following neurocentrum, as shown in plate

[5] Proceedings of the American Philosophical Society, vol. xvii, 1881, p. 523.
[6] Journal of Geology, vol. xi, p. 399.

12, figure 5, and by the narrowed lower extremity with the posterior superior margin of the hypocentrum, fitting into the angular space between the adjacent neurocentra. The large flat sutural surface for union with the preceding neurocentrum indicates a close, firm union, while that with the succeeding neurocentrum and hypocentrum is more rounded. The pleurocentrum of the first presacral vertebra is narrower than the preceding one. The pleurocentra of the two sides of each vertebra are closely approximated in the middle above a small concavity on the upper side of the hypocentrum, leaving in the articulated parts a persistent notochordal canal.

The zygapophyses are stouter in the free or lumbar portion of the column than in that part covered by the carapace, as would be expected, since the comparative rigidity of this part prevents extensive motion of the individual vertebræ upon each other. Their articular surfaces look uniformly upward and inward, and downward and outward, at an angle of something less than 45 degrees.

The diapophyses arising from the neurocentra increase rapidly in vertical extent of their rib attachment as far as the eighth, the border continuous with the parapophysial projection on the hypocentra. The ninth suddenly decreases in width, with a wide interval between its lower end and the hypocentrum, which has no parapophysial facet. From the ninth the transverse processes are narrower, with the extremity for rib articulation of but moderate extent. Throughout the series the processes are directed almost transversely outward, with the upper nearly horizontal margin a little thickened and rounded; the upper margin arises a little below the zygapophyses anteriorly, a little lower down posteriorly. The rib margin is straight or gently sinuous from above downward, thinned below and slightly emarginated, ending, as has been described in the first eight, in apposition with the parapophysial facet on the hypocentra. The lower end of the rib margin is considerably in advance of the upper. Beginning with the ninth, where the ribs become single-headed, the transverse processes are of nearly uniform width, the articular surface for the rib placed obliquely to the vertical line. Beginning with the seventeenth vertebra, the first behind the carapace, the diapophyses shorten rapidly, becoming almost sessile in the last two, in which the rudimentary ribs seem to be anchylosed to their extremities.

ATLAS

The atlas was preserved in this specimen in place. It is somewhat eroded and does not seem to differ from a better preserved specimen belonging to the closely related genus *Dissorophus*, which I have figured in

plate 14, figure 11. This vertebra seems to be a single element, though doubtless it is composed of coalesced hypocentrum and neurocentra; but I can distinguish no sutural lines. The anterior surface shows two facets for articulation with the occipital condyles. On the posterior side the body has a deep concavity, pierced above its middle by a small notochordal foramen. The neuropophyses are simple processes, of nearly uniform width, and flattened; they lie closely in apposition with the sides of the spine of the second vertebra. The same condition is found in *Eryops* and *Trematops*, and is doubtless the usual structure of the atlas in the rhachitomous amphibians. Back of the neurocentra I find no articular surface for the attachment of pleurocentra, though the anterior border of the next vertebra seems to indicate the presence of small pleurocentra.

The real composition of the atlas of the rhachitomous amphibians—for I doubt not that the vertebra is homologous with the atlas of the higher vertebrates—is a matter of some importance. If there be pleurocentra between the atlas and second vertebra, then we have the same structure as exists in the reptiles and higher vertebrates, the atlas composed of hypocentrum and neurocentra, the pleurocentra separated to unite with the axis. Against this interpretation, however, is the fact, as seen in the drawings, that the notochordal opening pierces the centrum precisely as it does in the later vertebræ—that is, between the pleurocentra and hypocentrum. If all three elements are coössified in the atlas, then it would lend support to the views held by some morphologists, of whom Broili has given the latest exposition, that the vertebræ of the amniota are composed of the combined hypocentrum, pleurocentra, and neurocentra.

The second vertebra of the series, which we may call the axis, because of its slight modifications in structure, has a smaller hypocentrum, as I have described, without parapophysial facets. The diapophyses are short and narrow at their extremity. The spine is much broader below than are the succeeding ones and pointed above, where it comes in contact at its extreme tip with the extreme front end of the carapace. On either side of the thinned anterior expansion of the spine below there is a slight depression, in which is lodged the slender flattened neuropophysis of the atlas.

SACRUM

(Plate 9)

As is well known, the reptiles and higher vertebrates, wherever they possess a sacrum, have invariably, or almost invariably, two or more

vertebræ composing it. And this is one of the characters which have been urged as indications of direct genetic relationship between the microsaurs, in which two sacral vertebræ are known to occur in some forms at least, and the reptiles. Hitherto, not only among recent, but also among extinct, amphibians, excluding the Microsauria, but one sacral vertebra has been known to occur, though I find certain references to sacra with two vertebræ among the older writers on the stegocephs. In *Cacops* we have two well developed pairs of sacral ribs broadly attached to the ilia. Of these, the first pair is a little larger and stouter than the second, though differing otherwise but slightly. The stout vertebral ends have two articulations, with a small non-articular surface between them, the upper and larger one attached firmly to the neurocentrum, the lower to the upper border of the hypocentrum, which again presents a parapophysial protuberance for its union. The somewhat crushed condition of the arches of the sacral vertebræ, as they were found lying in the pelvis, prevents the determination with certainty of the relations of the neurocentra to the hypocentra, but I suspect that they articulate on the sides with the ribs only and not with the hypocentrum. Beyond the articular head the stout shaft of the ribs is constricted for a short distance into an oval form, and then suddenly expands into the large flat or outwardly concave portion for union with the ilium. This thinned expansion of the first rib has an emargination on the upper posterior border, in which fits loosely the lower anterior border of the second rib, the two forming an elongated, nearly plane surface, which extends the whole length of the lower part of the ilium in its greatest width nearly opposite the upper part of the acetabulum. The first sacral hypocentrum is rather larger than the preceding one, with a parapophysial facet at each side for the rib. The second hypocentrum, of nearly equal size, has also a like facet for the rib on each side. The neurocentra of these two vertebræ are in part missing, apparently due to some accident before fossilization.

TAIL

The tail was preserved complete, but the flattened end was slightly disclocated, doubtless due to its thin, compressed form. The small bones of the neural side of the first six, or pygal, vertebræ were so small and so confused in the matrix that not much could be made of them. The six pygal hypocentra were found attached in a continuous series. There is a slight, very slight, possibility than an additional one may have been lost in the matrix at the place where the dislocation occurred, but I think not. The tail could not have been more than a fourth of an inch longer or shorter than is shown in the restoration and plate. The first six, or

pygal, hypocentra decrease rapidly in length. With the fifth is a short, rudimentary rib, a mere pointed tubercle, and several similar ribs were found loose in the matrix.

Beginning with the seventh, or possibly the eighth, caudal there is a continuous series of fourteen or fifteen with chevrons, some of them, as shown in the drawings, with the neural arches attached. It required a critical examination with a lens to distinguish the very small pleurocentra of the posterior ones; that I did distinguish them I am quite certain. The hypocentra are angular; the lower posterior part extended into stout chevrons, which were perforated near their base for the hæmal canal. The distal part of the tail was thin and high, possibly used as a rudder-like organ in life.

RIBS

(Plate 12, figure 8)

Many of the ribs have been removed from their encrusting matrix with but little or no injury; some of the posterior ones, because of their extreme delicacy, could not be worked out completely. The first eight pairs have broadly expanded proximal extremities, with a distinct separation, save of the first pair, of the rounded and thickened capitulum from the more elongated and thinner tubercle. The upper border is nearly straight or gently convex on the proximal portion, convex beyond; for the three distal fourths or more the shaft is slender, gently flattened, oval in cross-section, and is curved downward. Beginning with the ninth rib, the proximal expansion is much less, and there is no distinction into capitular and tubercular articulations; the shaft is more slender. The ribs increase gradually in length to the ninth and probably to the tenth. Beyond this they decrease more rapidly in length. In the last two pairs, at least, they are reduced to tuberculiform rudiments, ending pointedly, and are apparently coösified with the diapophysis.

From the tenth to the eighteenth only the proximal parts were worked out—that is, their precise lengths could not be determined.

SIGNIFICANCE OF HYPOCENTRA AND PLEUROCENTRA

Two views have been and yet are held by morphologists as to the morphological significance of the pleurocentra and hypocentra of the rhachitomous amphibians. The first, that of Cope and Baur,[7] is that in the evolution of the higher vertebrates the pleurocentra progressively developed to form the centrum, while the hypocentrum as progressively

[7] Cope: Transactions of the American Philosophical Society, vol. xvi, p. 243. Baur: Biologia. Centralbl., vol. vi, 1886, p. 12; American Naturalist, 1897, p. 975.

diminished, leaving a vestige only in the intercentrum of the modern *Sphenodon* and other reptiles, and possibly certain mammals. On the other hand, Cope, at least, believed that the pleurocentra disappeared in the holospondylous and modern amphibians, the hypocentrum persisting as the centrum.

The other view, that of various authors, notably Gaudry, Goette, Gadow, Jaekel, and Broili, is that the pleurocentra and hypocentrum fuse to form the centrum of all the higher vertebrates.

Unfortunately, both of these views must remain as hypotheses until, it is hoped, some fortunate discovery of intermediate forms may confirm or reject one or the other or both. In favor of the second view, which has been well presented by Broili,[8] it is seen (plate 12, figure 5) that the pleurocentra articulate with each other in the middle, inclosing below and between them and the hypocentrum a notochordal perforation, quite analogous, perhaps homologous, with the central perforation in all primitive holospondylous vertebræ. With a close fusion of the four elements of the vertebræ and obliteration of the sutures a rhachitomous vertebra would be practically indistinguishable from a notochordal vertebra of the early reptiles. Moreover, the presence of distinct elements in the primitive cartilaginous vertebræ of the chick, in addition to the arch, assumed to be pleurocentra and hypocentra, would suggest their persistence closely fused in the ossified centrum.

This explanation would seem to be the most reasonable of any, especially if we had to deal with the modern amniota alone. Unfortunately, there are objections, ones recognized by Cope and Baur, which complicate matters. In the first place, this theory would necessitate the recognition of a morphological distinction between the intercentra as originally so called by Cope, and as found in the living *Sphenodon*, and the primitive hypocentra. It is a well known fact that intercentra are far more prevalent among the old forms than the modern ones; that practically all reptiles with notochordal perforations possessed them, while only vestiges are found among modern reptiles save *Sphenodon*, leaving for the present the chevrons out of account. In the modern crocodilia alone has every vestige of them disappeared back of the atlas, while the lizards possess them throughout the neck, and turtles have at least one or two back of the atlas. If they are morphologically distinct elements, what explanation can be given for their origin and wide prevalence among the early reptiles and their gradual disappearance in the modern forms? They are known only in the amniota. Neither of the

[8] Monatschr. d. Deutschen Geologish. Gesellsch., lx, 1908, p. 236.

views as to the fate of the pleurocentra will militate against the commonly accepted theory of the structure of the atlas, where the united pleurocentra do not fuse with the hypocentrum, but remain distinct as the odontoid process of the axis. But we have in such forms as *Dimetrodon*, as well as all other Permian reptiles, to say nothing of the modern lizards, a very large element, often larger than the atlantal hypocentrum itself, intercalated between the atlas and the body of the axis below. If it be an intercentrum purely, it has assumed enormous proportions, suggesting very forcibly the original size of the intercentra throughout the column. If it be a hypocentrum like that of the atlas, we are irresistibly driven to the conclusion that either the body of the axis is its pleurocentra, or else that the pleurocentra and neurocentra of an intercalating vertebra have disappeared between the axis and atlas, leaving only the large hypocentrum. And if this hypothesis be true, then a like explanation would be necessary to account for all the intercentra, which certainly are morphologically identical with the intercalating element in *Dimetrodon*. But such a theory is altogether too tenuous for me. It may be all very well to account for the ossifications between the centra in *Sphenodon* as new elements originating from one knows not what and call them intercentra or subcentra, as Jaekel has very superfluously renamed them, but will some one give a reasonable explanation of the very large preaxial intercentrum in *Dimetrodon* and the other early reptiles, as well as the lizards, on this hypothesis?

Again, it is a remarkable fact, for which no reasonable explanation has ever been given save by Cope, that in all true amphibians, both ancient and modern, the chevrons are an integral, inseparable, exogenous part of the body of the vertebræ, while in all reptiles and higher vertebrates they are the intercentrum or part of it. In other words, they are in all rhachitomous and embolomerous amphibians a part of the hypocentrum, a prolongation of its lower part, perforated for the passage of vessels, immovably united, while in all amniota they are freely articulated between the vertebræ, or, as in some lacertilia, with the posterior part of the body itself. Free chevrons have never been discovered in true amphibians, and until they are we are compelled to assume that they are the hypocentra, and thus, under the second hypothesis, must be morphologically distinct from the chevrons of the amniota which are attached intercentrally.

It is a remarkable fact that intercentra have never been discovered in any amphibian, ancient or recent. In the Microsauria we have in some cases well developed vertebræ associated with a fully ossified skeleton, but so far no intercentra have been discovered in this group, so far as I am aware, nor free chevrons, save in *Hylonomus*. Again, the primitive rib

in the reptilia was articulated invariably with the intercentrum and neurocentrum, and never with the centrum exclusively. In the rhachitomous amphibia the articulation as seen in the present form was with hypocentrum and neurocentrum. Probably that was the invariable rule, for I know of no amphibian in which the capitulum of the rib articulated between the centra. This fact is suggestive, at least.

Yet, further, in the embolomerous forms it is the pleurocentra which become detached from the remainder of the vertebra to form an independent disk between the hypocentra, just as is the case in the atlas of the amniota.

I am fully conscious that the theory of the hypocentral origin of the centra of modern amphibians and the exclusively pleurocentral origin of the same of the amniota seems a bit improbable, though, of course, not impossible; I am not ready to receive it as a safe theory yet; I am still less ready to accept the theory of the independence of intercentra and hypocentra morphologically, as the other theory demands.

On a later page of this article I give the description and figures of a remarkably amphibia-like reptile, in which the vertebral structure seems to be intermediate between the rhachitomous and the reptilian type. It is not decisive, but is very suggestive.

PECTORAL GIRDLE AND EXTREMITY

Scapula coracoid.—The scapula coracoid (plate 10, figure 1; plate 11, figure 1) is a relatively large bone, with no indications of sutural division into its supposed component parts. The blade is moderately expanded above, gently concave on its outer, convex on its inner, surface. Its upper border is a little convex longitudinally, its edges sharply truncate for cartilage. The posterior border is thickened to the beginning of the glenoid concavity; the corresponding anterior border is thinned.

The posterior border divides to include the supraglenoid fossa, perforated at its bottom by the supraglenoid foramen. The outer border continues downward, and by a gentle curve backward to terminate in the oval preglenoid facet, which looks downward, backward, and outward. The inner margin, the thicker, extends downward, inward, and backward, with an anterior curvature. In one specimen the end is angularly truncate, in all probability for a small metacoracoid that was not ossified. In the others it continues in a thin border back of the margin of the posterior glenoid facet. This latter facet is near the lower part of the bone, an elongate concave surface, with sharp margins posteriorly, and is opposed to the anterior facet already described. Between these two facets and a little above them the glenoid foramen pierces the bone obliquely

backward to open on the inner surface a little back of the border of the subscapular fossa, in which the supracoracoid and the supraglenoid foramina both open. The lower anterior part of the bone is thickened, with truncate edges, and is convex in outline. The upper end of the cartilagious border continues backward as an angular thickening, nearly on a level with the upper border of the preglenoid facet. The thickening extends as a ridge a short distance backward. In much probability this ridge limits the upper border of the epicoracoid, since in the immature specimen already spoken of a sutural line seems to run directly backward below the upper edge of the preglenoid facet, quite as in *Varanosaurus*. The lower edge of the preglenoid facet continues as a rounded border downward and forward part way to the lower margin of the bone. In the cavity thus formed at the lower end of the facet is the opening of the supracoracoid foramen. On the inner surface of the bone, near the middle, extending downward subparallel with the anterior border of the bone, is the anterior border of the subscapular fossa. In its middle part the free border overhangs a deep fossa looking backward, at the upper end of which opens the supraglenoid foramen; at the lower end is the opening of the supracoracoid foramen. Back of this margin the bone is convex, and is pierced by the inner opening of the glenoid foramen.

Cleithrum.—The cleithrum, found in position on either side, is remarkable for its large size. Its lower part is a long rod closely attached to the anterior margin of the scapula as far as the angular thickening I have described; it is overlapped through nearly its whole extent by the upper part of the clavicle. The upper part of the cleithrum is broadly dilated and thin, covering the upper border of the scapula to its hind angle and arched inward. Its borders are very thin, convex above, concave below; the posterior thin margin is nearly straight. The bone above forms a sort of roof, convex outwardly, concave inwardly.

Clavicles.—The clavicles are small, somewhat spoonshaped, with curved handle. The upper, slender part is closely applied to the outer, anterior side of the lower, rodlike part of the cleithrum, reaching nearly to the dilated portion. The lower end, curved inward and a little backward, is dilated with thin margins, concave on the inner side where it articulates with the interclavicle, convex on the exterior or lower surface. It underlaps the interclavicle and partly covers the lower anterior border of the epicoracoid.

Interclavicle.—The interclavicle is a small, thin bone dilated and thinned in front where it overlies the ends of the clavicles, which touch in the middle. The posterior extension or "stem" is short, not as long as the expanded part.

Humerus (plate 11, figures 2-5).—The humerus is of the usual temnospondyl form, differing from that of *Eryops* and *Trematops* especially in the absence of the ectepicondylar process, so conspicuous in the former and in *Euchirosaurus*. It is broadly expanded at either extremity in planes nearly at right angles with each other, and has a rather slender shaft in the middle. The lateral or radial process is very stout; the medial or ulnar process is indicated by a slight ridge or elevation just below and in front of the inner extremity of the proximal articular surface. The digital fossa on the inner side is rather shallow and broad. The short shaft is subcylindrical in cross-section, with a sharp ridge running from the outer side of the lateral process to terminate in the supinator ridge. The capitular surface for the radius is subhemispherical in shape, looking mainly forward. The trochlear surface for the ulna is small. The inner, condylar border is moderately dilated and relatively thin; the ectepicondylar or supinator border thick. There is no indication whatever of the ectepicondylar process below the lateral process on the outer side.

Humeri of allied forms.—Among the early land vertebrates no part of the skeleton is more characteristic than the humerus. As a rule, the humeri of the amphibia and reptiles resemble each other markedly—rather stout and short bones, with the extremities greatly expanded and their planes divergent from each other in an angle of from 60 to 90 degrees. Though usually thickset and short, in some of the reptiles it is as slender as that of existing lizards, an example of which is seen in the humerus of *Pleuristion*, a small cotylosaurian reptile—a little smaller than the existing *Sphenodon*. Climbing reptiles have slender humeri, fossorial and aquatic reptiles thickset and short humeri, though in some amphibious forms, such as the crocodiles, it is rather slender. In strictly terrestrial, cursorial reptiles it is never short and stout, from which it may be inferred that the amphibians and reptiles from Texas with such thickset humeri are either fossorial or aquatic. But it is an inference only, since there may be other causes to account for the robustness, of which we are not yet aware. One conclusion is, however, justifiable: animals with such humeri as are shown in plate 15, figures 4 and 5, were certainly not quick running in habit!

As a very general rule, the humeri of amphibians and reptiles may be distinguished by the absence of an epicondylar foramen in the former; its frequent presence in the latter. Among living reptiles the crocodilia have neither; *Sphenodon* has both ectepicondylar and entepicondylar foramina. Lizards frequently have an ectepicondylar foramen or groove; even the mosasaurs possess such a groove, though no record of

it has hitherto been published. The Chelonia have, like the lizards, an ectepicondylar foramen or groove. Among the Amphibia an entepicondylar foramen was described by Cope in *Acheloma*, but an examination of the type specimen shows that while apparently present in one humerus, in its mate there is no indication of it, and the opening in the one may safely be ascribed to some accidental injury or malformation, especially so since its position and form are aberrant. So, also, *Euchirosaurus* has been restored with such a foramen, or rather with an ectepicondylar opening, but quite incorrectly so. The euchirosaurian humerus is of the strictly *Eryops* type, and like *Eryops* it does not have an epicondylar foramen. There are but two known amphibians, ancient or modern, with an epicondylar foramen, *Diplocaulus* and *Cochleosaurus*.

In the collections of the University of Chicago there are well preserved humeri of not less than ten genera of Texas amphibians. In addition to these I have examined the humeri of *Acheloma* and *Dissorophus*, and Broili and Case have figured the humerus of *Aspidosaurus*. Of four Permian genera the humerus is unknown—*Zatrachys*, *Anisodexis*, *Cardiacephalus*, and *Cricotillus*. The last-named genus is very doubtfully distinct from *Crossotelos*. *Anisodexis* is a large form, clearly allied to *Eryops*, and doubtless with a humerus similar to that of *Eryops*. *Cardiacephalus* is a very small amphibian, altogether too small to belong with any of the femora figured in plate 15. Of the remainder *Cricotus* may possibly be represented among the unidentified forms, but probably not, since nearly all the material in the Chicago collections are from the upper horizons, all of the humeri herewith figured, an horizon in which neither *Cricotus* nor *Zatrachys* occurs. One other genus may be mentioned, *Lysorophus*, of which evidence of limbs is found among the material in the collection, but the bones are small, almost minute. It thus is almost certain that we have evidence of at least fifteen genera of Permian amphibians from Oklahoma and Texas. I know of none from the reputed Permian of Illinois save *Diplocaulus* and *Cricotus*.

Of the humeri shown in plate 15 two certainly do not belong among the temnospondyles, those of *Crossotelos* (figures 2 and 3) and *Diplocaulus* (figure 7). Of *Crossotelos* I have seen a half dozen or more humeri from the Orlando bone-bed, always associated with vertebræ of the typical form and never with other forms. Its character was not recognized when first discovered, and the ends are unfortunately reversed in the drawings. It is a simple bone, moderately expanded at the extremities, with a rather deep concavity longitudinally behind, and with but a small lateral rugosity. All the specimens found show an incomplete chondral ossification. The form was doubtless more or less aquatic. *Diplocaulus*,

the only known amphibian, save *Cochleosaurus*,[9] with an epicondylar foramen, has been referred to the Microsauria or Lepospondyli by Cope, Broili, and Jaekel, as also by myself provisionally, but, as I pointed out in a recent paper,[10] the mode of attachment of the ribs is quite unlike that of the typical microsaurs and like that of the modern salamanders. Doctor Moodie has, because of this character, erected the new order of Diplocaulia for the genus, and I believe the name is valid, though the rank of the group is open to dispute. *Crossotelos* Case, a form with holospondylous vertebræ, found associated with *Diplocaulus* at Orlando, has distinctly double-headed ribs, with the capitulum attached to a facet just back of the front margin of the vertebral centrum, the tubercle to the extremtiy of a short diapophysis. True microsaurs have single-headed ribs attached intercentrally, with the chevrons presumably attached also as in reptiles. In both *Diplocaulus* and *Crossotelos* the chevrons are quite like those of the true amphibians, exogenous processes from the middle of the centrum. But I am quite confident that at least two distinct groups have been associated among what are called Microsauria, and that one of them, with single-headed, intercentral ribs and intercentral chevrons, must be dissociated into a group more nearly allied, possibly identical, with the reptilia in a wide sense, while the other will remain among the amphibia. As I am not able at the present time to decide which of these groups includes the type form of the order, it is premature to attempt the solution of the problem by giving new names. This thing, however, may be said with assurance, neither *Diplocaulus* nor *Crossotelos* can be united with such forms as *Eosauravus copei* Will. (nom. nov., *Isodectes punctulatus* Cope in part, *Isodectes punctulatus* Moodie), or *Sauravus costei* Thevenin. *Crossotelos* has numerous slender ventral ribs, very much like those of *Labidosaurus* and *Captorhinus* (*Pariotichus*), of which I have found abundant evidence in new material from Texas.

The humerus of *Trimerorhachis* (plate 15, figure 6) departs markedly from the ordinary temnospondyle type, as do also the femora, in which the adductor ridge is represented by a mere line on the ventral side of the femur quite as in the reptiles, and unlike all other known Texas amphibians. It is also remarkable for the small size of the lateral process, which is separated from the head by a considerable free space. This process is divided for the attachment of the several muscles into two rugosities opposite each other on the front and hinder surfaces. The median process, usually feebly indicated in the amphibians, is clearly

[9] Broili: Paleontographica, lii, p. 15, fig. 8a.
[10] Transactions of the Kansas Academy of Science, vol. xxii, p. 122.

seen in a small but prominent hooklike process on the inner posterior side near the head. The proximal articular surface is, it is seen, quite at the extremity, suggesting a limited range of motion as compared with other forms, and it is also the case with articular surfaces for the radius and ulna. Here, as in other cases, the absence of distinct condylar surfaces may be of generic value or merely ontogenetic. While, as is well known, many amphibians, both extinct and living, do not have a complete chondral ossification, leaving the articular surfaces throughout life composed of cartilage, yet it is also quite possible that such a condition found in fossilized bones may be merely indicative of juvenility. In *Trimerorhachis,* however, it seems certain that the character is one of maturity, since more than a dozen humeri and a score or two of other limb bones in the collection all present the same unossified condition. From all of which facts I think the conclusion is obvious that *Trimerorhachis* was an aquatic form, notwithstanding the slenderness of the humeri, which simply is an evidence of a short tail and limb propelling habits.

Three other humeri figured in plate 15 are certainly of new genera. Two of them, figures 4 and 5, are of the usual temnospondylous type, but a third (figure 1) may represent a new type of amphibian from Texas. This humerus, it is seen, is an unusually slender bone, with its articulations well developed. The lateral process is small and the condyles are feebly developed. The bone, as preserved, shows but a slight twisting of its proximal from the distal plane, which may be in part due to the conditions of fossilization, though I think not. The genus when it is better known will, I believe, be found to be of a slender terrestrial type.

The humerus shown in figures 4*a*, *b*, and *c* is remarkable for its massiveness and shortness, in both respects excelling any other which I know. Here also the chondral ossification is deficient, the articular condyles, both proximal and distal, being roughened, more or less concave surfaces, doubtless thickly covered by cartilage in life. The humerus shown in figure 5 was found associated with that illustrated in figure 4. It is more of the *Dissorophus* type, though distinctly different, as were also its femora found associated with it. It also has an imperfect chondral ossification.

All of the humeri so far described lack the peculiar process just above and to the outer side of the capitellar convexity, so characteristic of *Eryops, Euchirosaurus,* and apparently also *Trematops.* It may be called the ectepicondylar process, and seems to be homologous with a similar process found in the *Pelycosauria,* but hitherto unknown in the *Cotylosauria.*

In a future communication I shall give a similar comparative illustration of the humeri of the reptiles, of which there are in the collections of the University of Chicago 12 distinct forms, representing as many genera.

Radius (plate 14, figures 7-9).—The radius has a very slender shaft in the middle, and is broadly and thickly expanded at either end. The posterior border is nearly straight, the anterior deeply concave, the two lateral borders nearly symmetrically and deeply concave in outline, the extremities of nearly equal width and nearly transversely truncate. The proximal end has a groove on the posterior side thinning the inner articular surface.

Ulna (plate 14, figure 10).—The ulna is a remarkably slender bone in comparison with the radius—slender in comparison with the bone in other genera of Permian air-breathers. It has a slender and curved shaft on the distal three-fifths, the distal extremity only a little widened, with its greater diameter at right angles to the greater diameter of the proximal end. The olecranon is very slightly produced, and the articular surface for the humerus is oblique to both axes of the bones; the inner side of the proximal end is flattened.

Two bones of the proximal row of carpals with several phalanges were found close to the bones of the left forearm. They are relatively small. Their characters may be seen in the figures (plate 12, figure 3).

PELVIC GIRDLE AND EXTREMITY

Pelvis.—The pelvis (plate 12, figure 4; plate 13, figure 1) is very strong and stout, the two halves meeting in a very firm symphysis, which forms an obtuse ventral ridge most protuberant in the middle below the acetabulum. The pelvic cavity is deep and spoutlike, nearly semicircular in transverse outline, with the lateral margins anteriorly and posteriorly slightly flaring. The anterior border is emarginate in the middle, the sides convex in outline, with a notch. The posterior margin is slightly narrower than the anterior, and has a deeper emargination in the middle line, the sides somewhat thinner, with convex borders to the outer, somewhat angular margin. The posterior margin of each innominate is concave from the upper angle of the ilium, with a pronounced angular projection in the middle of the concavity at the junction of the ilium and ischium; this border is rather thin throughout. The front border is likewise concave throughout from the upper angle of the ilium, with a slight convexity below the middle at the place of junction between the ilium and pubis. This border is much thicker than the posterior, and flares outwardly below. The acetabulum is deep and large, with its greatest

concavity below its middle. It has a distinct and rather protuberant rim, save at the upper posterior part. Its upper border on the ilium is marked by a small but distinct process overhanging the concavity. The lower margin on the ischium is very prominent, forming an angular projection to the full width of the bone, while the stout expansion of the pubis in front limits the deepest concavity of the acetabulum. The shape of the acetabulum would indicate that the chief pressure of the femur was directed nearly horizontally and a little backward. The lower rim is nearly horizontal, deeply concave antero-posteriorly, overhanging the almost horizontal outer surface below. The pubes flare outward on each side in front from a subangular line, running downward and inward through the inner orifice of the obturator foramen to either side of the median emargination of the front border. The triangular surface either side thus limited looks at an angle of about 45 degrees upward from the horizontal position of the pelvis and slightly inward, and is gently convex from side to side. The under surface on the sides of the conjoined pelvis is nearly horizontal laterally, descending in the middle into a broad obtuse ridge, broadest and deepest a little in front of the middle. In front the downward curvature of the pubes leaves a concavity, at the bottom of which is the external opening of the obturator foramen, very near the middle of the pubis antero-posteriorly, and opposite the greatest protuberance of the pubic margin, not far from the border of the acetabulum. The front border of the pubes is roughened for cartilage. The sutures separating the elements are very clearly shown in the present specimen. Those between the ilium and ischium and pubes begin on the margins near the middle of the convexities described and run downward to meet a little below the middle of the acetabulum, that for the ischium being a little longer than the one for the pubis. The puboischiadic suture runs directly inward through the deepest part of the lower margin of the acetabular rim, the length of the ischia below being about a fifth greater than that of the pubes. The depth of the lower pelvic border is due solely to the breadth of the symphysis, the upper surface of the pelvis in the middle showing no corresponding concavity.

Femur (plate 13, figures 2-5).—The femur is remarkable among temnospondyles for its slenderness and the great development of the adductor crest. The proximal articular surface has its transverse diameter but little greater than the antero-posterior one, narrower on the outer side, more convex on the inner, where the articular surface extends more on the ventral side. The digital fossa is small and shallow. The adductor crest arises near the upper part of the bone, is directed outward for a short distance, and then is nearly straight to its distal end near the

lower fifth of the bone, and near the middle of the popliteal surface. The shaft of the bone for about the middle two-fifths is very slender, almost cylindrical, save for the crest behind, and is straight. The distal expansion of the bone begins a little above the lower fifth and is a little greater than the proximal one. The lateral linear concavities of the bone on the two sides are nearly symmetrical. The distal articular surface of the bone has sharp borders, indicating a considerable amount of cartilage. The end is much broader transversely than from before backward. The transverse tibial surface looks downward and a little backward and inward. The fibular surface is a little longer from side to side and looks markedly outward, backward, and downward. Its width in the inner side is more than one-half of the whole width of the extremity, with narrow extensions both in front and behind. The fibular condylar projection is much thinner and less extensive than the tibial. The extensor groove in front of the distal end is broad and moderately deep, limited on the outer side by a high and rather sharp ridge. On the back side a less prominent, more obtuse ridge opposite the dorsal ridge, and connected with the distal end of the adductor crest, separates a shallow concavity on the inner side from a narrower and deeper one on the outer side.

Tibia (plate 14, figures 1-4).—The tibia is more than three-fourths the length of the femur. Its upper extremity, as usual, is broadly expanded and massive, the lower less expanded and more cylindrical. The upper surface for articulation in the normal position of the bone is broad from side to side and about two-thirds as wide from before back, thicker on the outer than on the inner side, with an emargination on the outer anterior side, the anterior border internally convex, the posterior border nearly straight. The surface is gently convex from side to side, nearly flat antero-posteriorly, and looks on the whole upward and a little backward. The distal articular surface is suboval, its longer diameter running from behind outwardly and anteriorly, with the internal border convex, the outer posterior border more nearly straight or gently convex. The shaft is slender in its middle, broader in section from before backward, and is flattened on the inner side. The inner border of the bone is deeply concave, the outer almost straight, save at the lower end. The posterior surface of the bone is flattened on the upper expansion, bounded inwardly by a sharp sinuous crest, which becomes confluent with the convexity of the distal extremity.

Fibula (plate 14, figures 5 and 6).—The fibula is shorter than the tibia, flattened upon its posterior inner surface, and convex from side to side on the opposite. The outer thinner margin is nearly straight to the lower fourth, where it curves inward. The inner border is deeply

concave, more so on the lower half. The lower extremity is more expanded than the upper, and is also thicker, strongly convex in front, and somewhat concave on the posterior surface. In the vertical position of the bone the upper articular surface is nearly horizontal, while the lower is directed at an angle of about 20 degrees inwardly.

Associated with the leg bones were found a number of tarsal and phalangeal bones, figures of which will be found in plate 12, figures 1 and 2. Their precise position can not be determined, more than that two of the tarsals belong in the proximal row and three of the toe bones are metatarsals.

TAXONOMY

DISSOROPHUS MULTICINCTUS

From West Coffee Creek, in Baylor County, I was so fortunate as to find a considerable part of a skeleton of *Dissorophus multicinctus*, which adds not a little to our knowledge of this genus. The parts preserved are the nearly complete skull, with the palatal region imperfect, the nearly complete carapace, with several vertebræ attached, the nearly complete pectoral girdle, humeri, parts of the radius and ulna, the imperfect pelvis, a femur, parts of the tibiæ, and several tarsal bones. They will be figured and described soon by the writer. The skeleton agrees in its essential characters closely with *Cacops*, but is generically distinct in the much greater development of the carapace, with its fused anterior dermal bones, armadillo-like, and in the much greater stoutness of the appendicular skeleton. From this specimen and the material of *Cacops* may be given the following family and generic characters. In the family characters I italicize those based exclusively upon *Cacops*.

DISSOROPHIDÆ—FAMILY NEW

General characteristics.—Skull with the otic notch completely enclosed to form a large ear cavity. Palate with but two large teeth on each side, one at the anterior inner margin of the nares, the other at the posterior margin; mandibular and maxillary teeth of nearly equal size. Parasphenoid reduced. *Twenty-one presacral vertebræ; two sacral vertebræ; tail short.* A dorsal carapace, composed of lateral expansions of the spines of the vertebræ, with overlying intercalated dermal plates. Cleithrum very large and expanded above. Clavicles small, without exterior pittings. Interclavicle smooth on the dermal surface, small, with a short posterior process. Humerus without ectepicondylar process. Femur with strong adductor crest.

Genus Dissorophus Cope.—Dermal carapace extending the full width of the body, with a broad and elongated shield in front covering several vertebræ. Cleithrum less expanded and thicker. Scapula much expanded antero-posteriorly below. *D. multicinctus* (*articulatus*) Cope.

Genus Cacops Williston.—Dermal carapace but little wider than the vertebræ, narrowed in front and not fused into an anterior shield. Cleithrum thin and more expanded. Scapula less expanded below, the interclavicles and clavicles more slender. *C. aspidephorus* Will.

TREMATOPSIDÆ—FAMILY NEW

It is very evident that the characters of *Trematops* as given by me[11] are of more than generic importance. The genus represents a distinct family, which may be defined as follows:

A median foramen back of premaxillæ; large antorbital vacuities. Otic notch wholly enclosed by bone, the opening small and extending far forward. Palate with two pairs of large teeth back of the nares and a single one on each vomer. No parasphenoid. Ribs short, the anterior ones expanded distally. Twenty-two or twenty-three presacral vertebræ; a single sacral; tail short. No dermal armor or carapace. Cleithrum unknown. Clavicles and interclavicle small, without dermal pittings. Humerus with ectepicondylar process. *T. milleri* Will.

Restoration

(Plate 17)

The figures of *Cacops aspidephorus* given in the following plates and the mounted skeleton of plate 17 are nearly all from a single specimen, found with all of its parts intact and closely related in the matrix, lying in a prone condition. It was lying near the right side of one of the blocks of clay, taken up with the aid of bandages, and in separating the blocks some fragments of the right limbs were probably lost; the remainder probably are yet lying in the adjacent block, but the time required to work out each of these separate blocks, with their numerous skeletons and parts of skeletons, has rendered it inadvisable to wait till the whole material has been prepared before publishing, especially as but very little new information is to be gained. Inasmuch as the other skeletons exhumed are of a slightly larger or smaller size, their bones have not been used to replace the tibia and fibula, radius and ulna, and feet of the right side, which have been modeled in plaster from the left side. Numerous

[11] Journal of Geology, July-August, 1909, p. 389.

foot bones have been found dissociated, and probably a large part of the material must be worked over before the feet are found in all their natural articulations, inasmuch as it required the working out of six or seven feet and four or five hands of *Varanosaurus* from the same blocks before every detail of the structure of the extremities of that genus was determined. The foot bones thus have been arranged in the mounted skeleton after those of *Trematops* and *Eryops*, and it is quite certain there can not be much error. Nevertheless, it is expected that even these details will be determined in the course of a year or two. As is stated in the description, the precise lengths of the tenth to the eighteenth ribs could not be determined from the single specimen, as also the precise structure of the arches of three or four of the proximal caudal vertebræ. These have been restored, and it is not at all probable that the real bones when found will make any discernible difference in the mounted skeleton.

Everything else, to the smallest details of the mounted skeleton, is bone; the only plaster used was that necessary to cement the pieces together when broken in preparation. Furthermore, the posture of the skeleton is almost exactly that of the fossil as it was preserved in the matrix. Upon the whole I doubt whether there is another mounted skeleton of an extinct reptile or amphibian about which there is so little of error or with so little restoration as the present one.

The creature as mounted presents an almost absurd appearance, with its large head and pectoral region, absence of neck, and short tail. It is very certain that it possessed no other dermal ossifications than those of the median dorsal carapace, and it would seem almost as certain that the creature was aquatic or largely amphibious in its habits. Almost frog-like in appearance, it doubtless had more or less froglike habits. What the significance of the dermal carapace was I am at a loss to suggest. That it could have been of protection to the creature seems more than doubtful, whatever may have been its use in *Dissorophus*, where it covered the whole dorsal region. But this coincidence is remarkable: With an external turtle-like ear opening, it had also the beginning of a turtle-like carapace. And this parallelism is also seen in *Diadectes*, a reptile with dorsal dermal plates and turtle-like ears. It is quite possible that the toes may have been a trifle longer than they are shown in the mounted skeleton—that I hope to determine later, but it is not probable, judging from the considerable number of phalangeal bones that have been found with the skeletons. That the animal was a swimmer I do not doubt, and in all probability the feet were webbed—they were certainly not clawed. Whatever may have been the habits of the creature, it, with its nearly

related *Dissorophus*, must be classed among the oddities of vertebrate paleontology.

Desmospondylus anomalus—Genus and Species new

(Plate 16)

The specimens representing a peculiar type of reptiles, which I am constrained to regard as new, were discovered by Mr. Paul Miller within a few rods of our camp on West Coffee Creek, Baylor County, Texas, from about the middle portion of the Permian beds. A series of small vertebræ was found partly protruding from the clay, about 6 inches below another specimen of larger size, which was recognized as a species of *Trimerorhachis*. Many of the fragments of both specimens had been intermingled in the wash, and several hours were spent in carefully removing the clay containing them and washing out the fragments in the near-by Coffee Creek. The bones, fortunately, were firm and hard, and entirely free from matrix, though broken into many fragments. A study of this loose material discloses a large part of a skeleton of *Trimerorhachis* and numerous fragments belonging to the present species. The mass of clay taken up with bandages included the series of about a dozen vertebræ, with two humeri, right and left; two ilia, also right and left; a femur and two tibiæ, right and left, with a portion of a fibula and several phalanges. In addition a part of a large interclavicle was also found in the clay matrix. In the wash were found another humerus, a femur, two ilia, and a radius, and probably fragments of ulnæ and tibiæ, all of a slightly larger size, though otherwise agreeing closely with the corresponding bones found in position, or nearly in position, in the matrix. Whether this second specimen had been originally associated with the other in the same horizon, whether it was associated with the *Trimerorhachis*, or whether indeed it came from still a higher horizon, it is impossible to say.

In addition to this material belonging to one species, another femur nearly twice the size and more slender in form, though otherwise quite similar, was found a few weeks later associated with the remains of *Cacops*, described in the present paper. Its horizon was probably nearly that of the present species. From the famous bone-bed of Danville, Illinois, Mr. Gurley obtained, a good many years ago, a large and well ossified humerus which has long been a puzzle. Case figured this specimen, without assigning it to any known genus, in his paper on the Illinois specimens in the Journal of Geology, volume VIII, plate III, figures 4*a*, 4*b*. It has long been considered an amphibian, notwithstanding the

presence of an entepicondylar foramen. It unquestionably belongs in the present genus.

The University of Chicago collection of Permian vertebrates from Texas and Oklahoma now includes representatives of at least 26 genera, all of which I know more or less well. Five or six of the genera described from Texas and Oklahoma, some of them on fragmentary material, I have so far not identified, though I have seen the types of several. They are *Cricotillus* Case, of doubtful validity; *Seymouria* and *Cardiacephalus* Broili; *Isodectes*, *Pantylus*, and *Anisodexis* Cope. It is, hence, possible that our genus may be identical with some one of these, but the probability is so slight that I have no hesitation in giving the new generic and specific name to the present specimens.

The vertebræ, of which there are 20 or more centra and fragmentary arches, in addition to the connected series of 7 or 8, present some extraordinary characters—characters which are very suggestive of amphibian affinities, annectant between the rhachitomous and holospondylous types. The centra, coming all of them apparently from the posterior dorsal region and the tail, are short, almost disklike, deeply concave, with a small perforating foramen. The arches are entirely free; the sutural surface for their attachment is extensive, situated on the anterior three-fourths of the centrum and extending downward on the front margin to below the middle. Back of this sutural surface there is a similar beveled surface extending about one-fourth of the length of the centrum, which also reaches down on the posterior side to the middle of the centrum. The arches are very low, with a rudimentary spine only, resembling the arches of *Labidosaurus* or *Captorhinus*. The zygopophyses are very large and broad, with their surfaces nearly horizontal. Below and back of the anterior zygapophyses there is, on either side, a distinct diapophysis, on the more anterior vertebræ standing out prominently, on the posterior ones a mere rugosity. Lying by the sides of these processes were a number of small ribs, which seem to have been single-headed, inasmuch as no double-headed ribs were found in the matrix. However, as *Labidosaurus* has quite this form of diapophyses posteriorly with double-headed ribs, it is not impossible that such was the character of the ribs in this genus.

The anterior border of the pedicel, beginning low down, projects forward, so that if two vertebræ were closely applied the arch would rest on two centra, though chiefly on the posterior one, and, so far as I can determine from careful measurements, this would be the case with the zygapophyses closely interlocked.

That this was not the condition ordinarily, however, is rendered certain by the presence of extraordinarily large intercentra found in position

between several of the centra. Relatively, as compared with the centra, these intercentra are the largest known in any vertebrate, suggesting impressively the lower half of the pleurocentra of *Cricotus*. When in position they reach upward to the middle of the centrum, and almost or quite touch the extremities of the arch. (See plate 10, figure 3, and plate 16, figures 8-12.) If the ribs were double-headed the capitulum must have articulated with the upper ends of the intercentra. These intercentra are narrower above, so that there is left a distinct free space between the upper parts of the adjacent centra in the horizontal straight position of the column. When curved upward, however, the arches would fill the interstice between the contiguous vertebræ, leaving a wedge-shaped space below filled with the intercentrum. Some of the centra preserved are hardly more than half the diameter of the largest. They are evidently caudal vertebræ, though no indications of chevrons have been discovered. Others are even more disklike than the ones figured, resembling so closely various centra attributed to *Cricotus* from the Illinois deposits, that it is probable that they really belong in this genus and are centra, rather than to *Cricotus*, especially so as they agree in size with the femur mentioned above.

Not only are the vertebræ so curiously intermediate between the ordinary reptilian type and the embolomerous type, but the limb bones, both humeri and femora, were referred unhesitatingly to the amphibians before the vertebræ were recognized. The humerus (plate 16, figure 1) is extraordinarily stout and rugose for its length. Immediately below the lateral process there is a stout process, hitherto characteristic of certain temnospondylous amphibians, which I have called the ectepicondylar process, most characteristically seen in *Eryops* and *Euchirosaurus*. No such process is known in any Permian reptile, certainly in no Cotylosaurian. Furthermore, the median process is developed into a stout protuberance, quite as in *Eryops*. On the other hand, there is an entepicondylar foramen, remarkable for its large size, known only among amphibians in *Diplocaulus* and *Cochleosaurus*, wholly unrelated forms.

The femur (plate 16, figures 4, 5) also is remarkably amphibian in character in the extraordinary development of the adductor crest, a character known in no other Permian reptile. The digital fossa is extraordinary for its extent and depth, reaching nearly to the middle of the bone. The bones identified as tibia and radius (the former was found close to the femur and ilium, the latter in the wash) present no peculiar characters, though remarkably stout and robust (figures 2, 3).

Among the material in the wash are fragments of a small skull mingled with *Trimerorhachis* skull material, but there is too much doubt of their

reptilian character to make it worth while describing them until further evidence of their identity is forthcoming.

That the present genus is not a pelycosaurian is, of course, evident; its relationships with the cotylosaurians are more apparent. Nevertheless, the great differences in the structure of both vertebræ and limb bones from anything known among either the diadectid or pariotichid types render the exact position of the genus very doubtful. Possibly, as I have said, it may eventually turn out to be congeneric with some one of the few forms in which the vertebræ and limb bones are yet unknown, especially *Pantylus*.

Explanation of Plates

All the figures are by the author, from specimen number 647, and of natural size, save where otherwise noted

Plate 6.—*Cacops aspidephorus* Will. Top view of skull. Specimen number 649.

Plate 7.—*Cacops aspidephorus* Will. Palatal view of skull. Specimen number 649.

Plate 8.—*Cacops aspidephorus* Will. Figure 1, side view of skull; specimen number 649, except posterior part of mandible; figure 2, upper view of left mandible.

Plate 9.—*Cacops aspidephorus* Will. Vertebral column, from right, with sacral ribs.

Plate 10.—*Cacops aspidephorus* Will. Figure 1, scapula, with attached cleithrum and clavicle; *a*, from within; *b*, from without; figure 2, *Aspidosaurus* sp. Broili, spines of dorsal vertebræ; *a*, from the left; *b*, from in front; figure 3, *Desmospondylus anomalus*, posterior dorsal vertebræ; *a*, from the left; *b*, intercentrum from below; both figures twice natural size.

Plate 11.—*Cacops aspidephorus* Will. Figure 1, pectoral girdle from above; figure 2, left humerus, from inner side; figure 3, the same from outer side; figure 4, the same from in front; figure 5, the same from behind.

Plate 12.—*Cacops aspidephorus* Will. Figure 1, various toe-bones of left hind foot; figure 2, tarsals of same foot; figure 3, carpals and phalanges of left front foot; figure 4, pelvis, from below; figure 5, twelfth vertebra, from in front, the left pleurocentrum omitted; figure 6, dorsal shield of same vertebra; *a*, from above; *b*, from below; figure 7, hypocentrum; figure 8, ribs, as numbered, 3 and 5 of the left side, the others from the right.

Plate 13.—*Cacops aspidephorus* Will. Figure 1, pelvis, from the right; figure 2, left femur, from in front; figure 3, the same, from behind; figure 4, the same, from within; figure 5, the same, from without.

PLATE 14.—*Cacops aspidephorus* Will. Figure 1, left tibia, from in front; figure 2, the same, from behind; figure 3, the same, from within; figure 4, the same, from without; figure 5, left fibula, from behind; figure 6, the same, from in front; *a*, upper extremity; figure 7, left radius, from behind; figure 8, the same, from in front; figure 9, the same, from within; figure 10, left ulna, from in front; figure 11, *Dissorophus multicinctus*, atlas; *a*, from behind; *b*, from in front; *c*, from the side; *d*, from below; figure 12, anterior hypocentrum, twice natural size.

PLATE 15.—Figure 1, genus new, right humerus; *a*, from in front; *b*, from behind; figure 2, *Crossotelos* Case, left humerus, reversed, from behind; *a*, the same, from in front; *b*, the same, distal end; *c*, the same, proximal end; figure 3, *Crossotelos*, right humerus of another specimen, reversed, from behind; figure 4, genus new, right humerus; *a*, from in front; *b*, from behind; *c*, proximal end; figure 5, genus new, left humerus, from behind; figure 6, *Trimerorhachis* Cope, left humerus; *a*, from behind; *b*, from in front; figure 7, *Diplocaulus* Cope, right humerus, from behind.

PLATE 16.—*Desmospondylus* Will. Figure 1, *D. anomalus* Will., right humerus; *a*, from side; *b*, from in front; figure 2, *D. anomalus*, tibia; figure 3, *D. anomalus*, radius; figure 4, *D. anomalus*, right femur, from behind; figure 5, *Desmospondylus*, sp. n., left femur, from in front, natural size; figure 6, *D. anomalus*, fibula; figure 7, *D. anomalus*, left ilium, from without; figure 8, *D. anomalus*, vertebra, from in front; figure 9, *D. anomalus*, centrum, from behind; figure 10, the same, from in front; figure 11, the same, from above; figure 12, the same, from the left side; all figures, save 5, twice natural size.

PLATE 17.—*Cacops aspidephorus* Will. Mounted skeleton; specimen number 647; one-third natural size.

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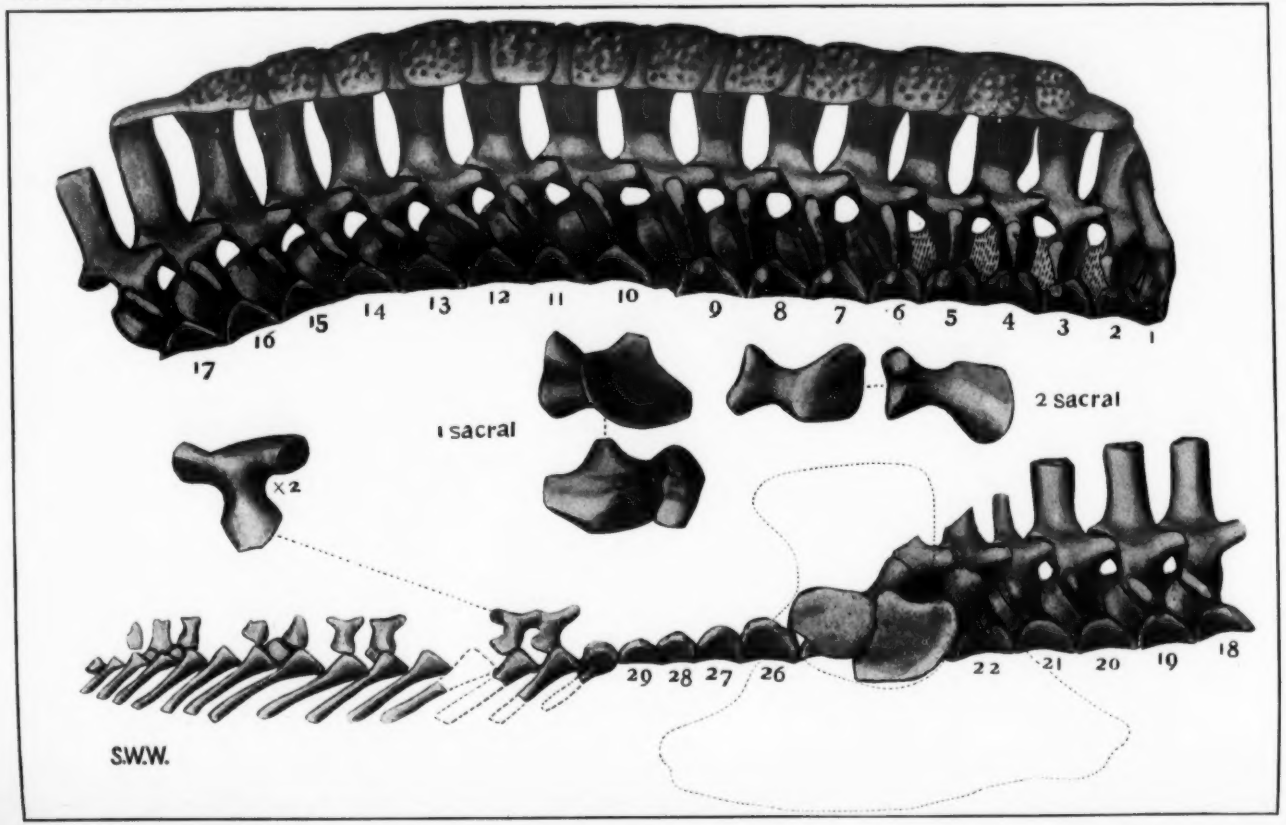
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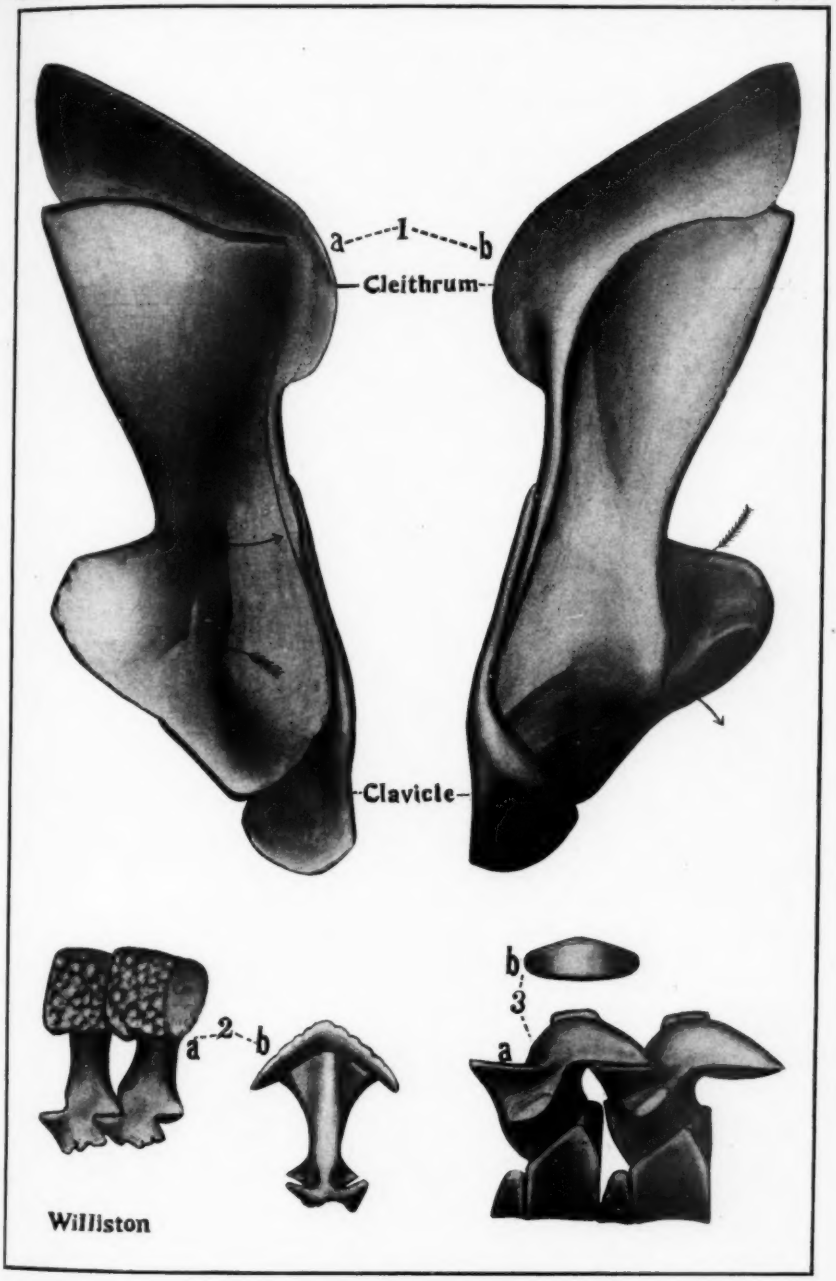

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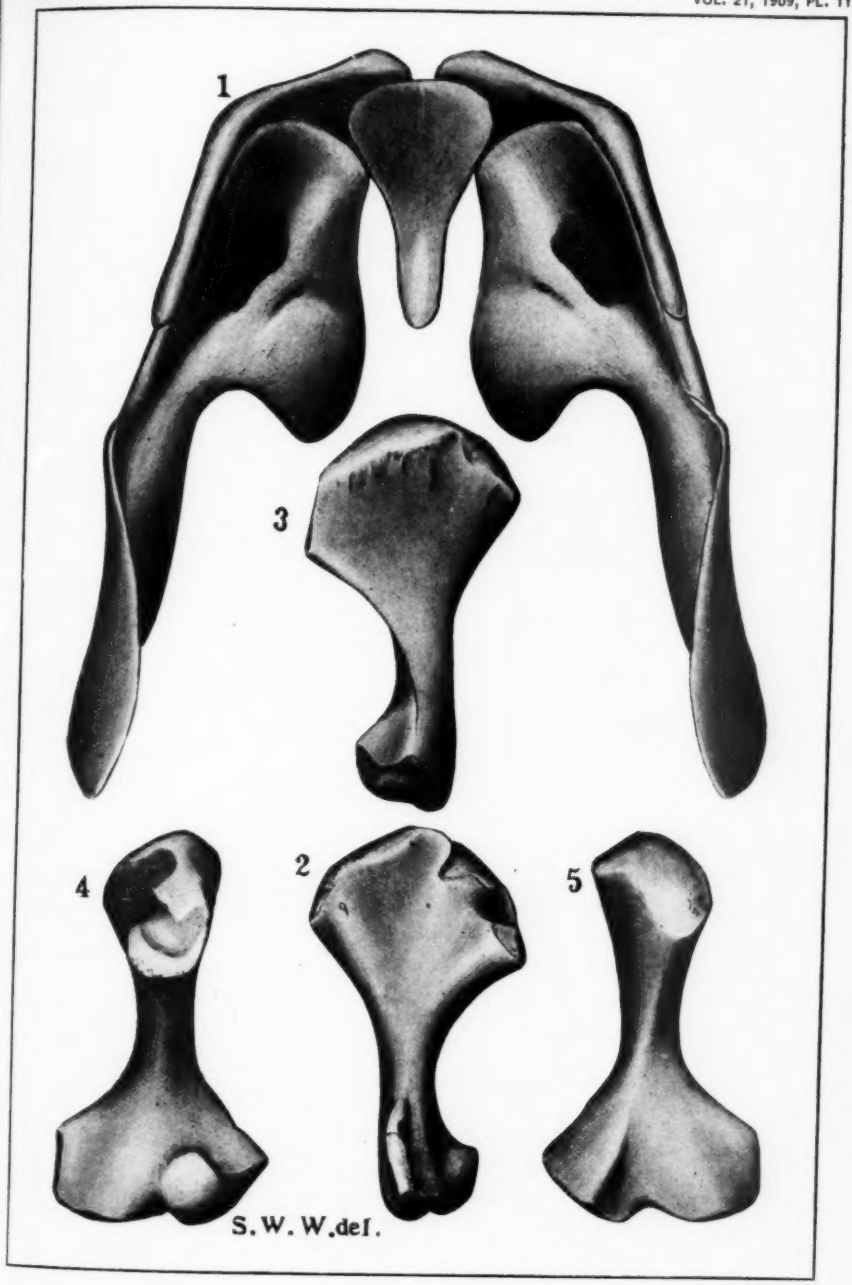

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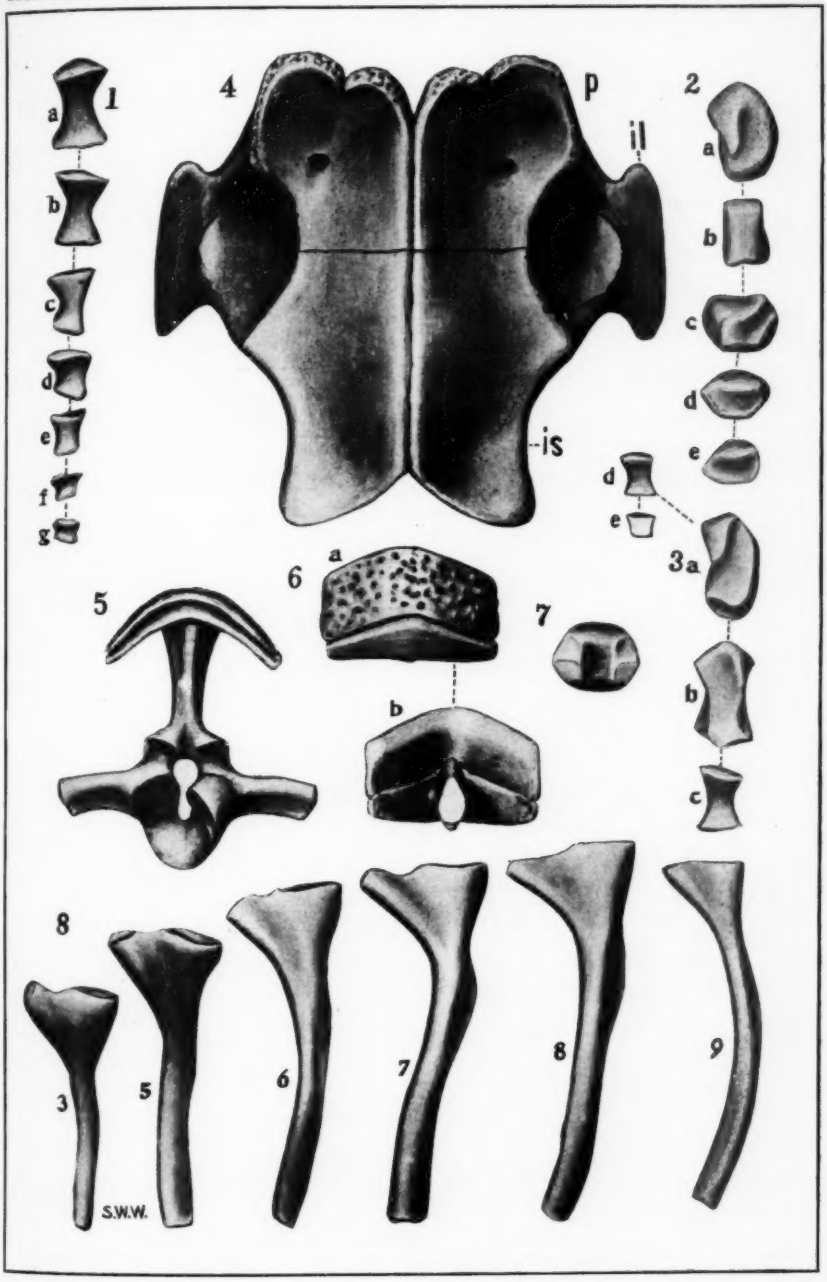

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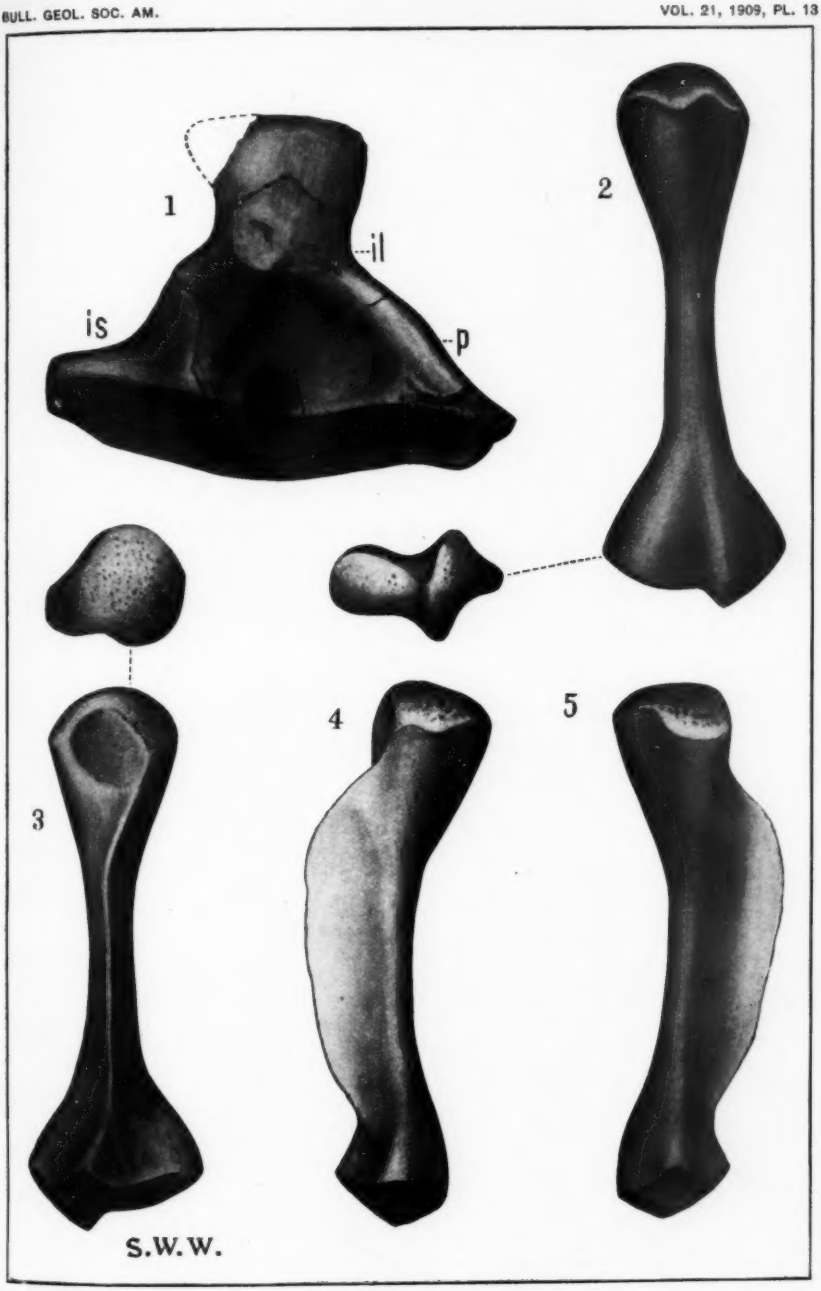

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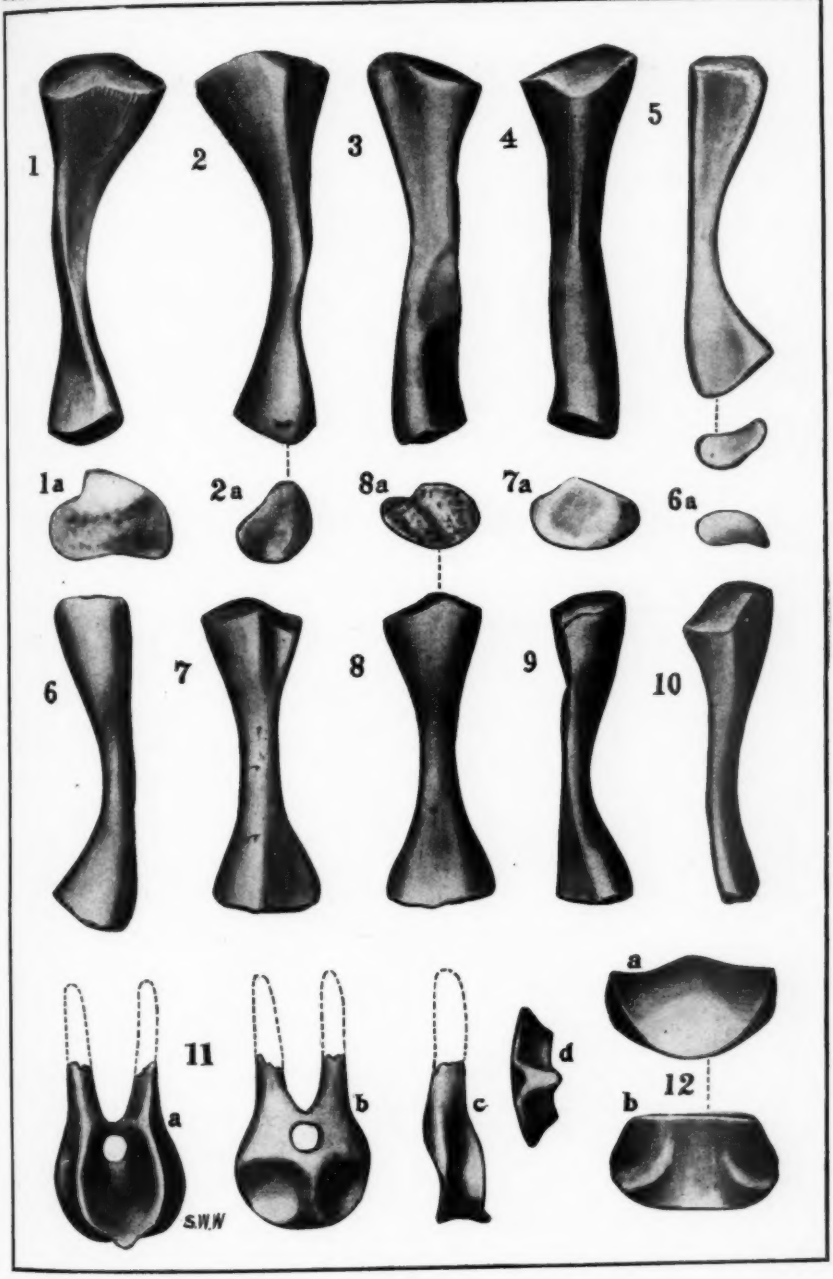

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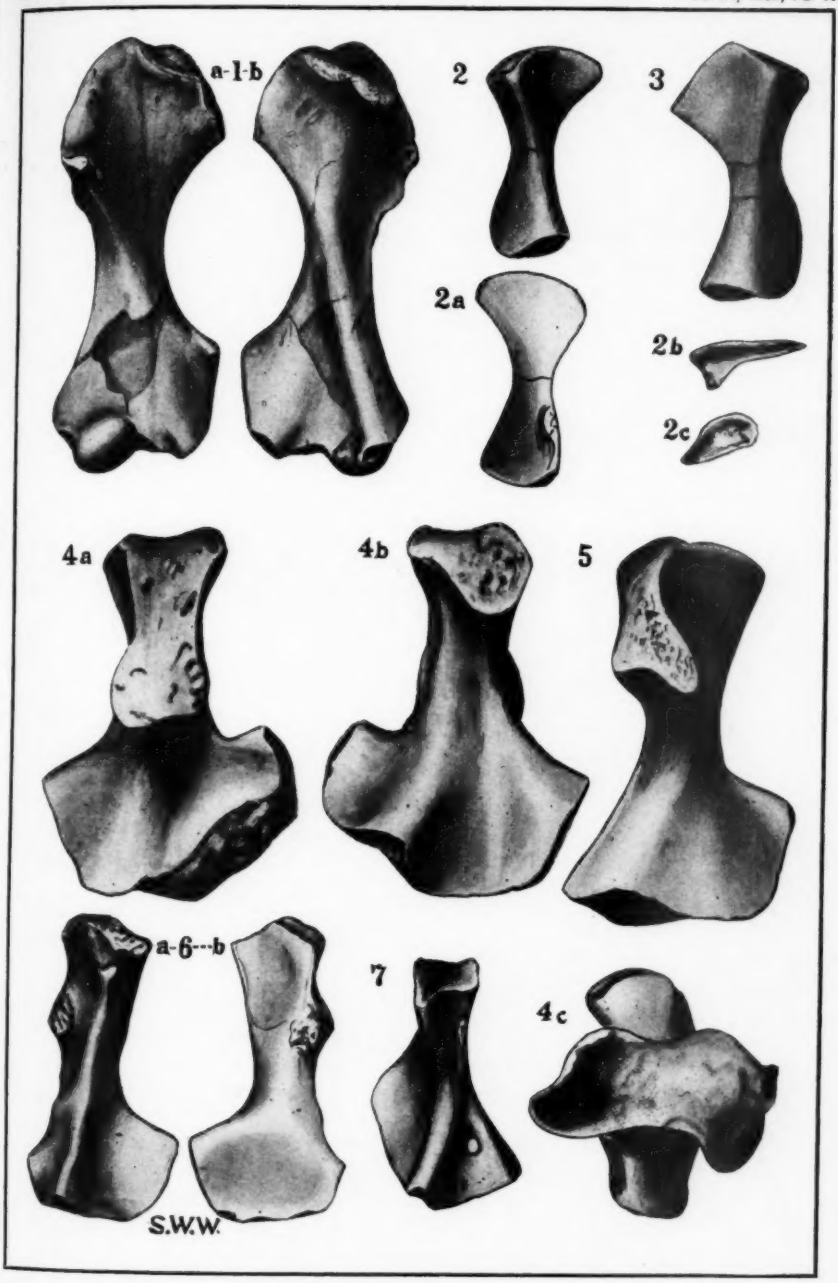

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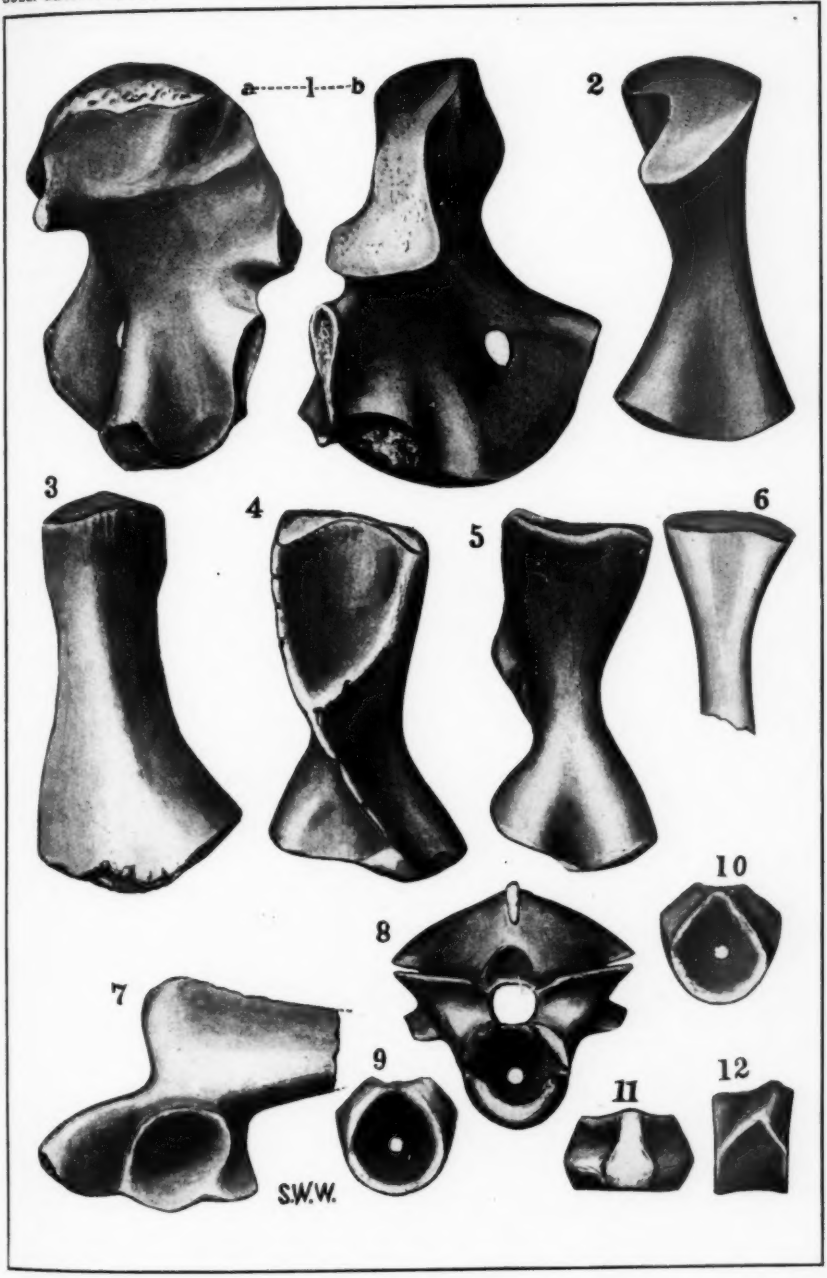

DESMOSPONDYLUS

CACOPS

BULLETIN OF THE GEOLOGICAL SOCIETY OF AMERICA
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MIGRATION AND SHIFTING OF DEVONIAN FAUNAS[1]

BY HENRY S. WILLIAMS

(Presented in abstract before the Paleontological Society Dec. 31, 1909)

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THE HYPOTHESES OF RECURRENCE AND SHIFTING FAUNAS STATED

In 1881 I presented before the American Association for the Advancement of Science the first definite announcement of the hypothesis of recurrent faunas, applying it to the fauna of the Marcellus, Genesee, and Ithaca black shales of new York, which I then conceived to be represented

[1] Manuscript received by the Secretary of the Geological Society April 23, 1910.

by the continuous fauna of the black shales of Ohio, Indiana, Kentucky, and Tennessee; and also, in the same paper, the hypothesis of shifting of faunas was applied to the Hamilton and Chemung faunas of central New York.[2] Since that time a large amount of evidence has been accumulated confirming these hypotheses. These hypotheses are intimately correlated. Recurrence or the departure of a fauna, its replacement by another, and its final reappearance in the same section at a higher level become the facts on which the hypothesis of shifting of the faunas is based; and the assumption of continuance and shifting of a fauna without losing its characteristics appears to be the only satisfactory explanation of its recurrence.

Facts on which the Hypotheses rest

The following facts explained by these hypotheses are among the more important which have come to light in the course of my studies.

Catskill sedimentation

This was shown to be thicker and to start lower down in the geological column in eastern New York than in middle and western New York. In eastern New York it began while the Hamilton marine fauna was still present and cut it off, bringing in estuarian conditions with a brackish water and land fauna and flora. In central New York no Catskill sedimentation is present until after the arrival of the Chemung fauna, and in western New York no trace of the Catskill type of sediments appears till after the close of the Devonian. These facts are direct evidence of shifting of the environmental conditions of the edge of the continent westward as the deposits of the middle and upper Devonian were being laid down. With this shifting westward of the off-shore conditions of the sea there went on a corresponding shifting of several faunas that were adjusted to each phase of those conditions.[3]

Reversal of order in succession of faunas

The appearance of the dominant species of the general fauna in reversed order of succession at the close of a fossiliferous zone.

The cases of *Spirifer lævis* in the Ithaca zone and of the frequent appearance of Leiorhynchus at the opening and close of a fossiliferous zone were among the earliest observed facts suggesting an actual shifting

[2] Proceedings of the American Association for the Advancement of Science, vol. xxx, p. 186, etc.

[3] "On the classification of the upper Devonian." Proceedings of the American Association for the Advancement of Science, vol. xxxiv, 1885, p. 222.

of the body of a fauna entering the area in one order of succession and departing in the reverse order.

In the Ithaca section there occurs at the base of the fossiliferous zone of the Ithaca member a bed containing abundance of *Spirifer (Reticularia) lævis*. The discovery of the same species at the top of the fossiliferous zone, as the normal Ithaca fauna become sparse, gave the first suggestion that the faunas were moving or shifting, the Reticularia zone marking the first trace of the fauna to enter and the last to leave the area. Confirmatory evidence was found in the order of succession of the dominant species of the Ithaca fauna. These facts were reported in 1883.[4]

The study of the mode of occurrence of Leiorhynchus still further drew attention to the definite order in which a series of species came in and went out of any given area. The species of the genus were generally found abundantly at the base or at the top of fossiliferous zones rich in brachiopods, in the midst of which Leiorhynchus was rare.[5]

RECURRENT HAMILTON FAUNA

The occurrence in a single or few strata of several representatives of an earlier fauna long after the formation to which they are normal has ceased.

Slight traces of this fact were observed in the first survey of the Devonian section passing through Ithaca,[6] and the fauna numbered 14 N (page 15) was called a "recurrent Hamilton fauna" because of the appearance there of such species as *Spirifer fimbriata, Sp. augusta, Pleurotomaria capillaria,* and others; and higher up, in the midst of the Chemung section, at Chemung narrows, *Tropidoleptus carinatus* and *Cypricardella bellistriata, Phacops rana* and *Dalmanites calliteles* were found.

The discovery of such traces of an earlier fauna led to further search, and as the evidence accumulated an elaboration and definite formulation of the theory of recurrence of faunas was made, which has been set forth in several papers, and is illustrated in detail in the folio of the Watkins Glen-Catatonk quadrangles, constituting folio number 169 of the U. S. Geological Survey.

ALTERNATE APPEARANCE OF DIVERSE FOSSILIFEROUS ZONES

The facts there brought out are substantially as follows:

There are exhibited in the sections mapped for the quadrangles *two*

[4] Bulletin 3, U. S. Geological Survey, p. 20, and Proceedings of the American Association for the Advancement of Science, vol. xxxiv, 1885, p. 222, etc.

[5] See Bulletin 3, U. S. Geological Survey, 1883, pp. 16 and 17.

[6] Reported in 1883, Bulletin 3, U. S. Geological Survey.

series of fossiliferous zones, the separate zones of the two series alternating in succession.

The zones of one series *dominate* the western sections of the area, and thus thin out or disappear on tracing them eastward. The zones of the second series dominate the eastern sections, and particularly the whole eastern New York sections, but thin out westward, and in some cases are entirely wanting in sections west of the Watkins Glen quadrangle.

The first set of faunal zones includes the faunas of the Genesee shale, the Portage formation, and the several divisions of the Chemung formation.

The second set of zones includes the Hamilton fauna, proper and recurrent representatives of that fauna, which I have named the *Paracyclas lirata* zone; the *Spirifer mesistrialis* zone; the *Leiorhynchus globuliformis*, or Kattel Hill zone (representing the typical Ithaca group of Hall at its typical sections at Ithaca); and the first, second, and third recurrent Tropidoleptus faunas, which I have called the Van Etten, the Owego, and the Swartwood Tropidoleptus zones.

All of these several fossiliferous zones of the second set become decidedly thin on passing westward across the region.

The Ithaca fauna is occasionally detected west of the Watkins Glen quadrangle, but is confined to less than 100 feet at Watkins, is recognized for 300 feet at Ithaca, and ranges through at least 600 feet along Tioughnioga River.

Only a slight trace of the Paracyclas zone is seen as far west as Ithaca, but is well expressed in the sections on the east side of the area.

The Van Etten, Owego, and Swartwood Tropidoleptus zones appear in thin tongues of strata as far west as the Waverly quadrangle and are seen in occasional traces as far west as the Elmira quadrangle, and when followed eastward appear to blend together as a modified Hamilton fauna, sparsely appearing in the strata up to the arrival of the Catskill type of sedimentation.

LIMITED RANGE OF RECURRENT SPECIES

Where the Hamilton recurrent zones are seen in sharpest exprssion the recurrent species range through only a foot or a few feet of strata, hold in abundance four or five characteristic Hamilton species, such as *Tropidoleptus carinatus*, *Cypricardella bellistriata*, *Rhipidomella vanuxemi*, *Spirifer marcyi* and *Delthyris mesicostalis*, cf. *D. Consobrinus*, and others, and the Owego and Swartwood zones appear in the midst of a characteristic Chemung fauna present both above and below each recurrent zone.

In the Owego recurrent zone both *Phacops rana* and *Dalmanites calliteles* occur.

The Van Etten recurrent zone lies entirely below the range of *Spirifer disjunctus* and other associated species of the Chemung formation.

On following the sections eastward from the Waverly quadrangle the species of the Chemung fauna become scarce, and east of the Chenango River very few species of the typical Chemung fauna have been detected, although they are still abundant in the Chemung rocks to the southeastward and southward across Pennsylvania, Maryland, and Virginia.

Interpretation of the Facts

Shifting of faunas

These facts have been interpreted as evidence not only of a general shifting of faunas coincident with a rising of the land along the eastern edge of the present continent, but of oscillation of conditions and alternate occupation of the area by two sets of faunas coming from opposite directions and temporarily living in abundance in the area of central New York.

Lithologic changes not sufficient to account for differences in faunas

The lithologic changes in the sediments containing the different faunas are not sufficient to account for the change in fauna. In quite a number of sections there is no appreciable difference in lithologic constitution between the strata which for 100 feet thickness have been filled with characteristic Chemung species and the immediately following thin zone of a foot or two, containing scarcely a trace of the Chemung species, but holding in great number species which if found by themselves would be undisputed evidence of the Hamilton formation.

Difference in ocean waters presumed

It becomes necessary therefore to suppose that the controlling cause determining the presence of one or other fauna is not the character of the bottom on which the sediments which preserved the fauna were laid. We are thus led to conclude that the character of the ocean water has determined the shifting or migration of the faunas. The conditions to which the faunas were adjusted were evidently those of depth, salinity, or temperature of the waters in which the species lived; and their change of habitation was occasioned by change in the direction, path, or extent of flow of the oceanic currents.

This leads us to consider the general principles of migration, and in particular those which affect marine organisms.

Migration of Species and Shifting of Faunas contrasted

Migration as commonly applied in natural history means the movement of large numbers of the same species from one place to another in a general definite direction at more or less regular periodic times. So birds migrate northward with the advance of warm weather; some fish migrate from sea to rivers in breeding seasons; pigeons fly eastward or westward in great flocks; grasshoppers invade a rich country, devouring the vegetation in their path, or lemming migrate in great hordes from mountain to lowlands.

The term in these cases has to do with movements of one kind of animal in relation to the comparatively fixed range of feeding ground for the remainder of the fauna inhabiting the areas concerned. The term is rarely applied to the slower movement of the whole body of animals of a fauna, coincident with great changes of climate such as the advance of the glacial cover over the northern parts of Europe or America produced during the Glacial age, or the advance of an Asiatic fauna across the Bering Straits and down the west coast of North America at some Pleistocene time, when an ice-bridge furnished means of communication by land from one continent to the other. Perhaps there is no impropriety in extending the application of the term migration to these latter cases in which the whole fauna and flora of a region are affected instead of a single or a few species, and in which the change of position of habitat is slow and spread over a great period of time instead of being coincident with annual change of seasons. The term may equally well be applied to movements in the seas and movements on the lands.

There is, however, one reason for choosing a separate name for the movements of the latter kind to distinguish them from typical migration: In the first class of cases the migration is voluntary and is performed by those organisms which have the power of more or less rapid locomotion. They may be said to do the migrating themselves. In the second case the movements are involuntary and the movement is forced on all the living organisms of the region. The change in position may be supposed to take place by the contraction on one side of an area of the conditions of possible existence for the species and the extension on the other side of favorable conditions of environment. The movements extend over many generations of life, so that relatively sedentary species may gradually adjust their *locus habitans* in the given direction of mo-

tion. To this latter process of migration I have been accustomed to apply the term "shifting of faunas."

Migration of species is in its typical sense an expression of the ability of some organisms to appreciate slight favorable changes in the conditions of environment and to take advantage of the better conditions during the lifetime of an individual. Shifting of faunas is an expression of the felt necessity, for the perpetuation of the race, of certain conditions of environment, resulting in the dying out of the whole fauna in the areas from which the favorable conditions have been removed and the spread of the fauna into new areas into which the favorable conditions have been shifted.

Shifting of faunas is an expression of the inability of any species of the fauna to survive under the changed conditions of environment which have overwhelmed them in their original habitat, but of an ability on the part of all those which migrate to follow the favorable conditions as they shift from one area to another.

Changed Environment Cause of both Migration and Shifting

In both typical migration of species and shifting of faunas, change in the environmental conditions of life constitutes the stimulus inducing change of habitat on the part of the organisms, and the movement of the organisms is a direct response to the stimulus. Those organisms in the first case which migrate show their greater vitality, compared with their neighbors who stay at home; while those who stay at home show the greater power of endurance and of organic adjustment to a wider range of environmental conditions than do those which migrate.

Close Adjustment of Species of a shifting Fauna

In the case of the shifting fauna, those species which endure without change of characters exhibit an acquired closeness of adjustment to some particular combination of environmental conditions which they are forced to follow or die and suffer annihilation. The evidence of their endurance is indicated by their return and reoccupation of the same area at a later geological stage when, by their reappearance, the original condition of environment is shown to have recurred.

Evidence of Migration

In the case of living organisms evidence of migration is found in the actual presence of the species at one time in a region at a considerable

distance from its ordinary *locus habitans;* and in some cases the species may be seen in the process of migration, as, for instance, the temporary alighting, in fatigued condition, of flocks of northern land birds on Bermuda Island on their migration southward.

In the case of fossil species the shifting of a fauna is recognized by the presence in a stratum of rocks of a number of species representing an earlier fauna surrounded by a different and, dominantly, later set of species.

Recurrence

The fauna is then said to *recur*, and it is the recurrence of the fauna which forms the basis for the inference that the fauna has shifted its *locus habitans* during the period of time represented by the sedimentary deposits separating the formation in which the fauna is dominant from the zone in the higher formation in which the recurrent species are found.

Theoretical Problems involved

This hypothesis of shifting of place and recurrence in time of a fauna involves certain conceptions as to the nature of species and the laws of evolution which it is important to consider.

Evidence of faunal Continuity

To establish evidence of motion in migration, as in any other kind of motion, it is all important to know that the body or bodies to which the motion is ascribed is continuously the same.

In the Devonian case the moving body is a fauna. Not only is it necessary to establish identity of the species in the recurrent zones with those of the initial zones, but it is essential to show that the faunas as a whole are the same. To put this in another mode of statement, we must establish the fact that not only the individual species have retained their specific characters, but the further fact that the equilibrium of adjustment to each other in the faunal community has not been changed, in order to prove that a supposed recurrent fauna is actually the direct successor of a fauna represented in the rocks at a lower horizon.

Rare and dominant Species

This has led to the distinction between rare and dominant species. Only as the comparative frequency of the species in the faunal combination is maintained can we be sure that we are not considering an acci-

dentally accumulated sample of a general fauna instead of the successive appearance of a special fauna.

The presence of an occasional associated species belonging normally to the fauna of the formation in which the recurrent zone appears is not antagonistic to the hypothesis, because the hypothesis proposes an invading of the territory already occupied by a normal fauna; and in case the currents or other causes which brought about the shifting of the fauna were not so completely different as to annihilate all evidence of the fauna previously occupying the ground, some few species might be supposed to hold over. Hence it is only necessary to find an abrupt change of the majority of species to make the induction that the faunas have shifted their habitats.

Magna Faunas and local special Faunas

The theory involves the further conception of grand general faunas or magna-faunas which have their center of habitat and distribution in permanent oceanic basins, as distinguished from the local, special and (in geological strata) temporarily expressed faunas, such as we are accustomed to associate with individual geologic formations.

In the case before us two such magna faunas are in evidence, one of which in its dominant characteristics is traced westward into Iowa, Idaho, and Arizona and up the Mackenzie River valley to the north, and across the polar regions to Russia and northern Europe. The other is traced eastward and southward into central and southern Europe, and also appears dominantly in South America.

In a case of recurrence in which there has been continuous sedimentation it is practically impossible to distribute all the species according to their source of origin. I have found it possible, however, to distinguish a few species as undoubtedly derived from a source different from that of the prevailing fauna characterizing the beds both below and above the recurrent zones.

Fixed and fluctuating Characters

It is only by close examination and comparison of the fossils themselves that identity of species or identity of faunas can be established.

The fixed characters are not only those characters by which one species is distinguished from another, but they include others of generic, ordinal, and even class rank, which may be of immense age in the race and mark no special narrow stage of its history.

It is a question of interpretation whether each particular phase of expression of fluctuating characters is a matter of time or of environment.

I have reached the conclusion that it is those species whose characters have the greater degree of normal and persistent fluctuation which migrate and follow the shifting conditions of environment, and their life period is correspondingly longer.

On the other hand, species whose plasticity of characters is narrow are more closely adjusted to their environment, are local in their range of habitat, and are temporary in their geologic life period.

Interpreting the facts on this basis, it is the phases of continuously fluctuating characters in species of wide geographic distribution and of long geologic range which furnish the most satisfactory evidence of temporary stages in the life history of faunas.

Difficulties in establishing Evidence of strict Contemporaneity

Another question of interpretation arises when we attempt to reconstruct the physical condition of the environment at successive stages of time.

In a single vertical section we have positive evidence of succession in time. If we were sure that no recurrence of the same fauna could take place, we could correlate two vertical sections strictly upon the fauna contained in the strata—on the hypothesis that the single fauna existed but once, and when it ceased in a given section its whole life period had been expressed. But the facts show us that this is not the case in nature. In geological times, as in the present, we know that many distinct faunas are living on the face of the earth at the same time, even for very similar conditions of environment. It becomes, therefore, a very complex matter to establish the fact of contemporaneity or to correlate two sections in which the order of faunas and the character of the sediments differ, which is generally the case for any two sections separated by 50 miles from each other, although on stratigraphic evidence they may be properly interpreted as covering the same interval of time.

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PERSISTENCE OF FLUCTUATING VARIATIONS AS ILLUSTRATED BY THE FOSSIL GENUS RHIPIDOMELLA[1]

BY HENRY S. WILLIAMS

(Presented in abstract before the Paleontological Society Dec. 31, 1909)

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Introduction

The following paper is the result of investigations which were begun several years ago, under the inspiration of Huxley's prophecy that paleontology, when properly interrogated, would reveal to us the true laws of evolution. I have at last succeeded in getting together a long series of samples of a single race inhabiting successively the same region; have subjected them to minute measurement, and by reducing their morphologic characters to mathematical terms have enabled them to speak for themselves regarding at least one of the essential laws of evolution, the law of fluctuating variation.

Inadequacy of vague descriptive Terms

One of the greatest difficulties met with in making such an investigation is the practice of using extremely vague and elastic terms in describing the specific characters of fossil organisms. Generic characters are in many cases made more definite by the fact that the diagnostic characters are either present or absent or the repeated parts are found in definite numbers; but in distinguishing species the use of such terms as short, long, broad, thick, deep, high, oval, ovate, oblong, etcetera, which are very commonly applied to both the gross morphology and the description of each distinguishable part of a fossil, stand directly in the way of all

progress in the discovery of the minute steps by which modification of form is accomplished in evolution.

Application of biometric Method

My paper is therefore not only a statement of some facts, but an application of the biometric method to the interpretation of fossils.

The genus *Rhipidomella* Oehlert was selected for first application of the method, almost at random among a number of similar series, chiefly because as a morphologic form it can be easily and certainly distinguished from all associated genera, and, further, because it offers clear and distinct characters for measurement.

Prerequisites to the Study of Evolution of Fossils

Before it is possible to interpret the laws of evolution from a geological series of specimens so that the results can be regarded as scientifically established, a rigid control of the conditions of selecting the samples is essential. The samples must all be of one genus; the fauna in which they occur must be unchanged and continuous; the length of time and the change of environmental conditions must be sufficient to furnish the opportunity for evolutional modification; the effects of geographic distribution and of artificial selection of the specimens must be eliminated and some mode of mathematical expression of the facts must be devised which will not only be exact, but will eliminate the changes of dimension incident to ontogenetic growth of the individual.

Faunal Continuity

In order to obtain a continuous series of samples of the same genus, running through a period of geologic time long enough to satisfy the conditions for the evolution of new species, it was necessary to establish faunal continuity, for the reason that where there is a faunal break of continuity in succession there can be no certainty that the specific representatives of the same genus in the two faunas have the same origin. They may have had widely different origin and represent evolutional races whose ancestry had been distinct for long periods of preceding time.

Environmental Change sufficient to permit evolutional Modification

Again, in order to establish the fact that the genus had suffered a sufficient amount of environmental change to give it an occasion for

evolutional modification, it seemed necessary to trace the genus through more than a single stage of its life history, for the reason that any morphologic differences expressed by a genus under a single set of environmental conditions can be interpreted only as fluctuating variations within the limits of specific characters and may have nothing to do with evolutional modifications.

Effect of geographic Distribution

In order to be sure that geographic distribution did not enter in to complicate the evidence, it was necessary to collect the samples from a series of successive beds within a limited area within the confines of the same geologic basin.

Artificial selection of Evidence

Further to insure that no artificial selection of evidence take place, uniform conditions of collecting the evidence and a very thorough search, and the subjecting of every trace of the evidence to examination were necessary conditions.

Biometric Method

And, finally, because of the elastic and indefinite nature of descriptive terms applied to form, it was necessary to reduce the characters under examination to mathematical terms, so that they could be accurately stated and compared. I made all measurements myself, and with the same instruments, in order that whatever personal equation might enter into the statistics the errors would be reduced to a minimum by being uniform for all the statistics.

Location and geologic Horizon of Specimens

It has taken a number of years to satisfy all these conditions (I started the investigation in 1881), and in August last I began, for the first time, to make the measurements, the results of which I now have to report.

The geological sections from which the specimens were obtained are all within an area in central New York within a radius of 50 miles from Ithaca as a center.

Stratigraphically, the zones range from the middle of the Hamilton formation, through the Genesee, Tully, Portage, and to the top of the Cayuta member of the Chemung formations, presenting a total thickness of approximately 2,300 feet, which is at least 1/100 part of the thickness of the whole sedimentary beds of the earth's crust from which distinct

evidence of organisms has been obtained. In time, according to various estimates, this represents from 270,000 to over 1,000,000 years.

GEOLOGIC SECTION

THE ZONES AND INTERVALS

The samples of the genus come from five distinct zones in the following order:

Chemung	Swartwood zone: Interval of 400 feet.
	Owego zone: Interval of 400 feet.
Portage	Van Etten zone: Interval of 900 feet.
	Ithaca zone: Interval of 600 feet.
Hamilton	Hamilton zone.

HAMILTON ZONE

In the Hamilton formation of Cayuga Lake the genus Rhipidomella ranges through the upper 300 feet of the rocks (according to Professor Cleland's careful elaboration of the facts, given in Bulletin 206 of the United States Geological Survey), and for the upper 200 feet it is a common species in almost every fossiliferous zone. The samples subjected to exact measurement were obtained from the middle of this zone, perhaps 200 feet below the Tully. The whole of the Tully limestone, the Genesee black shale, and the Sherburne member of the Portage, each with a distinctive fauna, intervened between the first and second zone from which our specimens were obtained.

ITHACA ZONE

The second set of samples of the recurrence of Rhipidomella were obtained in a single layer in Buttermilk Creek south of Ithaca by Dr. E. M. Kindle. They lie below the main fossiliferous zone of the Ithaca member of the Portage formation, fully 600 feet above the first horizon of the Rhipidomellas of the Hamilton. About 900 feet of strata, holding the typical Ithaca fauna of the upper or Enfield member of the Portage, intervene between the Ithaca and Van Etten zones.

VAN ETTEN ZONE

The zone in which the next sample of the genus Rhipidomella appears is about 900 feet above the Ithaca zone in (what has been called and

mapped in the Watkins Glen-Catatonk folio) the first recurrent Tropidoleptus zone. This zone is typically represented in the section at Van Etten and contains Rhipidomella in 17 localities in the area examined. This zone is in the upper part of the Enfield member of the Portage, below the first appearance of the Chemung fauna in the section.

OWEGO ZONE

Four hundred feet above the Van Etten zone the Owego, or second recurrent Tropidoleptus, zone appears. This zone is about 200 feet above the first appearance of the Chemung fauna and 400 feet below the top of the Cayuta member of the Chemung.

SWARTWOOD ZONE

This zone is the third recurrent Tropidoleptus zone of the folio and lies at the top of the Cayuta member. In this particular area no *Spirifer disjunctus* has been discovered above the Swartwood zone.

Recurrent Tropidoleptus Fauna in the Watkins Glen-Catatonk Folio

Careful search has been made of every fossiliferous section over the 8 15-minute topographic quadrangles making up the area of the Watkins Glen-Catatonk folio, and in 34 localities samples of Rhipidomella have been obtained, all in association with other species representing the general recurrent Tropidoleptus fauna. In some cases only two or three specimens are in evidence; in the majority of cases, however, from 10 to 12 or more good specimens for measurement are in evidence.

Distribution of Sample Sets by Zones

The number of different sample sets is as follows for the several zones:

Ithaca	2
Van Etten	17
Owego	6
Swartwood	9

For the Hamilton zone a large collection of specimens obtained by H. H. Smith from the shore of Cayuga Lake furnishes the specimens for the series here recorded. The figures given by Hall of the type specimens of the Hamilton species of New York State were also measured for comparison. In all there are 35 sets of samples, each set from a particular

stratum of rock at a single locality. Each of the sets except that from the Hamilton is from a zone of rock above the Hamilton, containing Tropidoleptus and other characteristic Hamilton fossils, but at such a position in the rock section that its exact stratigraphic position is fully established. The association of species in each case is sufficiently large and distinctive to establish the recurrent nature of the fauna.

Between the zones mentioned no trace of the genus Rhipidomella has been found anywhere in the area studied.

All the specimens gathered were examined, and all those sufficiently perfect to furnish the measurements needed were tabulated.

Number of Specimens measured

The total number of specimens measured was nearly 300. Those furnishing both length and width were 277, distributed by zones as follows:

	Specimens
Hamilton	20
Ithaca	24
Van Etten	115
Owego	40
Swartwood	78

Measurements and Ratio Indexes

Each one of these specimens was measured by vernier calipers reading to 1/10 of a millimeter, for (*a*) its length from point of beak to middle of front margin furnishing index L, and (*b*) its width across from side to side at place of greatest width giving the index W.

The ratio between length and width obtained by reducing the value of index W to its percentage of the value of index L formed the ratio index R^1.

One hundred and forty-two specimens of pedicle valves showing the interior and the muscular scar were measured.

These were from the zones as follows:

	Specimens
Hamilton	12
Ithaca	0
Van Etten	48
Owego	26
Swartwood	56

For each of these the values of the length (index L^1) and the width (index W^1) of the muscular scar were obtained, and then the measures

were also reduced to the percentage ratio of width to length to obtain the index ratio R^3.

The index R^2 was obtained by comparing the length of the muscular scar with the length of the shell; this ratio reduced to percentage was ratio index R^2.

Reasons for Selection of Rhipidomella for Study

A word of explanation may be here offered as to the reasons for selecting the genus Rhipidomella and the particular dimensions for measurement. Rhipidomella was chosen because it offered a number of distinct morphologic characters which at a glance could be seen to fluctuate, as well as others which seemed to be repeated with great precision and were alike for all the specimens. The specimens were also well preserved or else left clear impressions when the shell was dissolved from the rock.

There was no other genus represented in these rocks so closely related to Rhipidomella as to furnish any doubt as to the proper reference of the specimens to the genus. Schizophoria, in young specimens, might possibly be confused with young specimens of Rhipidomella if only the exterior were in evidence; but no case arose in which the generic relations were not evident. Also Rhipidomella is fairly well represented in number of specimens for the majority of the faunules showing the recurrent Tropidoleptus fauna.

The particular characters and dimensions chosen for measurement were selected because of their fitness to furnish accurate measurements and because they well represent the general and particular fluctuating characters of the shells.

Form of Shell, Ratio Index R^1

It was observed that the various described species of the genus differ most conspicuously in the shape of the shell. While this shape is made up of a large number of details, differences in any one of them appear to affect the proportion between width and length, and for this reason these dimensions were chosen for measurement. By taking these two diameters and reducing them to a percentage scale, a ratio index was obtained which expresses by a single number the particular form fluctuation of each specimen on a common scale, whatever the size of the shell. This ratio index R^1 furnishes, therefore, not only a mathematical expression for the shape of an individual shell, but it is a means by which

differences in shape can be readily compared, both in amount and direction of fluctuation, with mathematical precision.

Muscular Scar of Pedicle Valve

The other character chosen for measurement was the muscular scar of the pedicle valve. This is a conspicuous, peculiar, and fluctuating character of the genus, and as it is well defined on good specimens and molds of the interior, it furnishes a valuable second character for precise study.

The muscular scar differs considerably in form and size in the various described species of the genus, and these differences, of whatever kind, have more or less direct effect on the proportion between width and length. Hence each scar was measured for length and width, and from the two measurements the value of index R^2 was obtained, which expresses in a single mathematical term the percentage ratio between the width and length of the scar.

Proportionate Rate of Growth of Parts

A third relation, also expressing differences in the rate, the proportionate rate in different parts, and the direction of growth, was obtained by estimating the percentage ratio between the length of the muscular scar and the length of the shell; this percentage gave the value of index R^2 for the several shells.

Ratio Indexes R^1, R^2, R^3

METHOD OF PROCEDURE

Thus were obtained three ratio indexes, represented by the values of R^1, R^2, R^3. The values for each index should be alike for any two shells presenting no variational differences; and the difference in value of the indexes for different specimens expresses, on a common scale, the kind and degree of difference in form, directly in three ways and indirectly in a large number of ways not easily describable otherwise. If the shell changed its form on passing from young to adult state, comparison of the values of their indexes for small and for large specimens will show it. Hence the law of ontogenic growth can be distinguished from and compared with the law of the phylogenic modification by comparison of these ratio indexes.

Although I caught something of the drift of the evidence as I was preparing the statistics, I deliberately put the specimens away in drawers, after all the measurements had been recorded, and used only the recorded

value of the three ratio indexes, without attempting to interpret them into characters, in estimating the following results.

RESULTS

Explanation of the ratio indexes.—The chief results are expressed in the mathematical values of the three ratio indexes R^1, R^2, R^3:

R^1 stands for the form of the shell;

R^2 stands for the proportionate size of the muscular scar in relation to the size of the pedicle valve;

R^3 stands for the form of the pedicle muscular scar.

The index L stands for size of shell expressed in millimeters;

L^1 stands for size of the muscular scar in millimeters.

1. *Size of shells—Values of index L.*—(*a*) The average length of 277 specimens from all zones is 19.1 millimeters.

(*b*) The mean of averages for the 35 separate faunules is 19.8 millimeters.

(*c*) Average length of the separate specimens of each zone by zones is, for 20 Hamilton specimens, 18.2 millimeters; for 24 Ithaca specimens, 17.5 millimeters; for 115 Van Etten specimens, 18.6 millimeters; for 40 Owego specimens, 20 millimeters; for 78 Swartwood specimens, 19.8 millimeters.

Uniformity in average size.—(*d*) This shows a remarkable uniformity in the average size of the specimens; the value of L varies from 17.5 to 20—that is, 2½ millimeters.

Here it should be noted that special directions were given to the collectors to make no selection in collecting specimens, but to gather specimens as far as practicable to represent the actual relation of abundance and rarity shown by the several species and the range in variation in size and other characters expressed by each species for each separate faunule.

Length of shell increases on passing upward.—(*e*) Examination of result (*c*) above shows that there is a slight increase in the average size of the shell on pasing upward. This is in harmony with another fact not brought out by the statistics. The thickness of the shells shows a decided increase on passing upward. In the Hamilton and Ithaca zones the shells are mainly frail and often crushed in fossilization. In the upper zones rarely are any specimens crushed, and there are frequent specimens showing thickening at the margin and strengthening of the processes about the hinge and bordering the muscular scars.

Size alone not significant of evolution.—(*f*) The size of the shell alone, however, can not be used as an index of evolution, because each

shell has a differing size index for different stages of its own individual growth; and, secondly, because size of specimens of any particular faunule is determined partly by the stage in individual growth at which the specimen died, but chiefly by the *rate of motion* of the currents which selected the particular shells at the point of their fossilization.

2. *Size of muscular scar—Values of index R².*—(2*a*) The average of R^2 for 142 specimens for all zones is $R^2 = 64.1$.[2]

(2*b*) The average for the specimens of each zone by zones: For 12 Hamilton specimens is 60.7; for the Ithaca, no evidence, 0; for 48 Van Etten specimens is 64.8; for 26 Owego specimens is 63.2; for 56 Swartwood specimens is 64.7.

(2*c*) In order to catch more fully the ontogenic law of development of this character, it is necessary to note the fluctuation in value of the index R^2.

For the Hamilton set the amount of fluctuation is from 47 to 71 per cent = 24 per cent.

For the Van Etten set the amount of fluctuation is from 57 to 82 per cent = 25 per cent.

For the Owego set the amount of fluctuation is from 56 to 72 per cent = 16 per cent.

For the Swartwood set the amount of fluctuation is from 52 to 78 per cent = 26 per cent.

Phylogenic increase in size of muscular scar.—This shows a slight phylogenic increase in proportionate size of the muscular scar, both in the lower and higher limits, namely, from 47 to 52 and from 71 to 78.

Examining this character for one of the earlier representatives of the genus, namely, *Orthis hybrida* Hall of the Niagara, we find in the type figures the index $R^2 = 56$. Two characteristic Coal Measure forms at the other extreme of the evolutional history of the genus give $R^2 = 63.3$ (for *Orthis pecosi* Marcou) and $R^2 = 70$ (for *Orthis penniana* Derby); difference, 56-70 = 14 per cent. This shows that the phylogenic tendency is decidedly toward an increase in the length of the muscular scar with time, represented by 14 per cent between the Silurian and Carboniferous samples.

Fluctuating value of index R² for three type species of the Hamilton.—Again, if we examine the type specimens figured to represent the several Hamilton species, the fact appears that *Orthis leucosia* may normally vary in value of R^2 from 69.2 (figure 4*h*) to 83.9 (figure 4*i*), or 14.7 per

[2] The number 64.1, as the value of R^2, means that the muscular scar averages 64 and 1/10 per cent of the length of the shell, and so in all expressions of the value of R^2.

cent; that *Orthis penelope* may differ from 46.8 (figure 2*g*), 56.3 (figure 2*i*) to 61.6 (figure 2*h*), or 14.8 per cent, and that *Orthis vanuxemi* (figures 3*m* and 3*r*) may normally differ from 69.2 to 74.8, or 5.6 per cent.

Comparison between ontogenic fluctuation and phylogenic modification.—These statistics show a range in value of this index, for the Hamilton types alone, from 46.8 to 83.9, or 37 per cent, which exceeds in both extremes the extreme limit reached by any of the specimens measured in our series; it also exceeds the limit of fluctuation of this character expressed by the average value of R^2, as recorded by typical figured specimens of characteristic species from the early and latter ends of the life history of the genus.

To bring out this point more distinctly, we note the difference between the extremes in percentage values.

The extreme fluctuation in value of index R^2 between typical Niagara and typical Carboniferous forms is 14 per cent; for typical figured specimens of Hamilton species, 37; for typical specimens of the species *O. leucosia*, 14.7; for typical specimens of species *O. penelope*, 14.8; for typical specimens of species *O. vanuxemi*, 5.6; for measured specimens from a single Hamilton faunule, 24; for measured specimens from the Van Etten zone, 25; for measured specimens from the Owego zone, 16; for measured specimens of the Swartwood zone, 26.

But the total amount of change in the average value of index R^2 from the Hamilton to the Swartwood zone is only 4 per cent.

It seems evident from these statistics that the difference in value of the character R^2 is from 4 to 6 times greater for any set of specimens selected at any particular stage in the history of the genus than the difference in value shown by two samples separated by a period of time represented by the sedimentation of 2,300 feet of sediments in the passage from the Hamilton formation to the top of the typical Chemung.

This law of the persistence of fluctuating variation is equally well indicated by comparison of the evidence of fluctuations for each separate faunule.

3. *Shape of the shell—Values of index R^1.*—The statistics regarding the shape of the shell are found in the measurements of 275 specimens, representing 35 separate sample faunules, and from 5 distinct stratigraphic zones. The morphologic character involved is one of the most conspicuous as well as important of the characters used in specific diagnosis.

If the average of the values of the index R^1 of all the specimens of each zone is taken, the following results are found:

	Average.	Extremes of fluctuation.	Amount of fluctuation.
For 20 Hamilton specimens	111.9	104.5–118.2	13 7
21 Ithaca zone specimens	108.1	99.2–125.5	26.3
115 Van Etten specimens	109.3	73.4–147.8	74 4
40 Owego specimens	105.6	91.0–119.5	28.5
79 Swartwood specimens	108.2	91.3–137.3	46.0
Average for the whole set	108.2		

From these statistics it is evident that the amount of phylogenic modification of this character R^1 (105.6 to 111.9 = 6.3 per cent) is extremely small in comparison with the ontogenic fluctuation (see last column in above table) for any one of the zones examined.[3]

Amount of ontogenic fluctuation great.—The amount of the ontogenic fluctuation is brought to light in another way by tabulating the differences in value of the index R^1 for each separate sample lot and computing the average for the whole 35 lots.

There are 35 lots of specimens, each lot from a single locality and faunule. The range of fluctuation in value of the index R^1 for each lot is shown by the following summary:

7 lots show no variation, because there is but a single specimen in evidence	0
6 lots show a range of fluctuation less than	10 per cent
6 lots show a range of fluctuation between	10 and 19
11 lots show a range of fluctuation between	20 and 29
1 lot shows a range of fluctuation between	30 and 39
3 lots show a range of fluctuation between	40 and 49
1 lot shows a range of fluctuation of	58
The average amount of fluctuation in value of R^1 for the 35 lots is	17

The fluctuation of this character expressed by the type figures of several described species is as follows:

	Amount of fluctuation.	Mean.
Orthis cyclas Hall	110.8 – 117.1 = 6.3 per cent	113.9
Orthis idoneus	107 – 113.8 = 6.8 per cent	110.4
Orthis leucosia	103.5 – 112 = 8.5 per cent	107.7
Orthis penelope	106.7 – 121 = 14.3 per cent	113.8
Orthis vanuxemi	107 – 111.8 = 4.8 per cent	109.4

and the total difference in form expressed in values of R^1 for these five Hamilton species, as above enumerated, based on measurement of the

[3] Phylogenic modification is the amount of morphologic change distinguishing the representatives of one stage from those of another following stage of its history; ontogenic fluctuation is the change in form expressed by different individuals of the same race living at one and the same time.

type figures, amounts to 103.5–121 = 18 per cent—that is, the extreme difference expressed by the type figures of the five species of the Hamilton is only 1 per cent greater than the average difference for each lot of the 35 lots of measured specimens.

Seventeen of the 35 sample lots collected in the field show a greater amount of difference in this character (index R^1) than the total amount of difference between the extreme examples of the type specimens of all the species of this genus described from the Hamilton for the New York area.

The difference between the mean values of index R^1 for the Hamilton type figures is only (107.7–113.9 =) 6.2, and 22 of the 35 samples show a wider fluctuation than 6.2.

Nevertheless, the greatest amount of difference between the average values of R^1 for the several zones, taken by zones, is (105.6–111.9 =) 6.3, and both extreme average values taken by zones are less by almost exactly 2 per cent than the actual extremes expressed by all the typical Hamilton species.

Phylogenic modification slight.—The statistics show a slight decrease in value of index R^1 on passing upward from the Hamilton to the upper Chemung zone, which fact is in harmony with the general phylogenic movement for the whole evolutional history of the race, as shown by comparing two Silurian species with two characteristic Carboniferous species:

	Value of Index R^1.	Average.
Orthis circulus of the Clinton	117.3	116.6
Orthis hybrida of the Niagara	116.0	
O. pecosi Marcou, Coal Measures	86.2	86.8
O. penniana Derby, Coal Measures	87.5	

showing a decrease of about 30 per cent on passing from the Silurian to the close of the Carboniferous (Pennsylvanian).

Phylogenic modification in same direction as ontogenic growth.—This phylogenic decrease in the value of index R^1 is also in harmony with the ontogenic law shown by comparing the young with the adult stage of a single specimen. The young forms are always broader than the adult, which would be expressed by higher percentage in the value of R^1 for young than for adult stages of growth.

Are the fluctuating characters specific or varietal?—In order to test the question whether these fluctuations in R^1 are truly varietal or may result from mixing several distinct species, the following facts have a bearing, namely:

The set of specimens selected for measurement from the Hamilton zone all belong to the one species *Rhipidomella vanuxemi* of Hall. (See Cornell University Museum Catalogue number 9139.) Twenty shells were measured for the statistics. The extremes for index R^1 for these 20 shells are 103.9 and 125.5, which shows a range of fluctuation of this index of 21.4 per cent for these 20 specimens. These 20 specimens were preserved in the same rock and bear such close resemblance to each other that I think scarcely any paleontologist would question their being one species. They are distributed in number of specimens in the following way by value of the index R^1:

2 specimens have index $R^1 = 100$ to 105
4 specimens have index $R^1 = 105$ to 110
8 specimens have index $R^1 = 110$ to 115
3 specimens have index $R^1 = 115$ to 120
2 specimens have index $R^1 = 120$ to 125
1 specimen has index $R^1 = 125$ to 130

showing emphatically that no two specimens in this small set of samples are of exactly the same shape; and, while the specimens are more numerous near the average, the difference in R^1 is such that at least 10 of them are separated by at least 1 per cent from the next specimen in the series.

This fact of positive and constant fluctuation is noticeable for all the separate lots, taken either by single faunules or by zones. It is also noticeable when the values of R^1 are tabulated for each of the Hamilton species on the basis of the type specimens as figured for each species.

Are the Hamilton "species" natural or artificial groups?—In order to test the question as to whether the described Hamilton species are natural species or only artificial groups, I determined the value of R^1 for each of the 37 specimens figured as types of the 5 species described by Hall as *O. cyclas, idoneus, penelope, leucosia,* and *vanuxemi.*

I found the range in value of this index for the whole set is 103.5 to 121 = 17.5 per cent and the average about 109.

Taking the 7 points (from 106-113), we find 23 of the 37 type specimens come within this range and at least half of the types of each species except *O. penelope* have the form expressed by this limited range in value of R^1, distributed as follows:

Two are *O. cyclas,* 2 are *O. idoneus,* 4 are *O. leucosia,* 4 are *O. penelope,* and 11 are *O. vanuxemi.*

Outside of this mean range are found 4 specimens below 106, all of which are *O. leucosia.*

Ten specimens are above 113, of which 2 are *O. cyclas*, 1 is *O. idoneus*, 5 are *O. penelope*, and 2 are *O. vanuxemi*.

It is evident from these statistics that the specimens selected as types of the 5 Hamilton species have no constancy in the value of R^1. Distribution of the type specimens in the order of value of R^1 show this character to be a constantly fluctuating character for each so-called species, and not one of the species is strictly discontinuous in respect of this character from each of the other sets of types, although selected as types of specific groups; from which the conclusion is drawn that, so far as this character is concerned, the so-called species are artificial rather than natural groups.

The mean of the fluctuations.—It is further evident from examination of the statistics that the mean of these fluctuations for the Hamilton is near 109, and the mean does not vary by 4 points either side of 109 for any of the zones in the upper Devonian.

The total range of fluctuation in a fair set of specimens from any local faunule from one end of the series to the other rarely is less than 20 points, and while on passing upward there is a clear tendency to increase of the range of this fluctuation and a drop in its mean value, the amount of this phylogenic change is but a fractional portion of the ontogenic fluctuation observed in every sample in which a couple dozen of specimens are in evidence.

Conclusions

For the present paper it is unnecessary to go into further details. The examination of the shape of the muscular scar, as expressed by the index R^3, shows the same law reported for the other indexes.

All three indexes, R^1, R^2, and R^3, point to the same conclusions, namely:

(*a*) The characters under examination are fluctuating variations in the Hamilton stage.

(*b*) Sample sets of specimens taken from the recurrent faunules of the Ithaca, Van Etten, Owego, and Swartwood zones exhibit the same characters as fluctuating variations.

(*c*) The fluctuations affect all the representatives of the genus without regard to any arrangement of the specimens into artificial specific groups and express themselves in sets of specimens derived from a single faunule or sets of faunules from a single zone.

(*d*) There is in the successive zones evidence of a tendency to move in a definite direction on passing from lower to higher geologic horizons, and this phylogenic modification is, in each case, in the same direction as the modification, expressed by individuals in their ontogenic growth.

(*e*) The expression of these characters in these Devonian samples of the race is clearly intermediate between their expression in typical samples of the race taken from the Silurian and from the Carboniferous respectively.

(*f*) It appears reasonable to generalize these facts to form the statement that phylogenic modification follows the law of ontogenic growth. This appears to me, after making these studies, to be a more correct form of statement than to say, as we have been accustomed to do, that the ontogeny is a recapitulation of the phylogeny. From the facts it seems more probable that the direction of evolution is determined by the laws of individual growth than the converse, so far as there is any relation between the two.

(*g*) The prominent fact thus brought out, namely, the persistency of fluctuation of characters for such a long line of descent, suggests a new conception of the nature of organic variation.

The behavior of fluctuating variations indicated by this study of Rhipidomella indicates that the fluctuating is probably an effect of the fluctuating environment reacting on characters which are transmitted without fluctuation.

To use an illustration: Suppose a number of buildings were constructed according to a definite plan, in which the exact number and order of arrangement of the bricks of the walls was determined. The houses thus constructed would be alike if the bricks were of rigid material; but if the bricks were made of rubber or elastic material, the lines of the house would constantly vary with the load put on them. The conception suggested by these facts is that the plan of construction for each organism is transmitted by heredity with the same degree of definiteness for fluctuating as for unfluctuating characters, but that the reason why some characters are fluctuating is because the materials of construction are susceptible to the influence of environment during growth, and hence reflect directly the influence of the fluctuating stresses of environment.

(*h*) This elasticity of the materials of construction enables the organism to adjust itself to different environment in its growth, but the adjustment does not in any degree affect the plan of construction or whatever it may be which is concerned in the transmission of characters in heredity.

(*i*) This variability is akin to the motion of movable parts of an organism, and concerns hard parts only where they come into direct contact with environment or with active (muscularly) soft parts of the structure.

(*j*) These fluctuating characters are as important to the organic economy of the organism as are the rigid, hard parts, and are as essential in the constitution of the species; as it might be said that the joint of the

elbow is as essential a part of the specific structure in making up the mechanism of a limb as are the rigid bones of the arm. The range and direction of movement provided by the flexibility of a joint is as much a specific character as is the shape of the rigid bones.

So we may generalize these facts and say that the range and direction of the fluctuating characters constitute an essential part of the specific constitution of an organism, and according to the statistics here presented appear to be transmitted with the same persistence from generation to generation as are the non-fluctuating characters which we are accustomed to regard as indicating their specific rank by ceasing to fluctuate.